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DAMASCIUS

LECTURES ON THE PHILEBUS WRONGLY ATTRIBUTED TO OLYMPIODORUS TEXT, TRANSLATION, NOTES AND INDICES

by

L. G. WESTERINK

The anonymous commentaries on the Phaedo and the Philebus which were appended to the collected commentaries of Olympiodorus about 900, have long been an unknown quantity in Neoplatonic studies. Though they are commonly referred to as the work of Olympiodorus, they are evidently far superior to the standard that prevailed at Alexandria in the sixth century. Recent research has pointed towards Damascius as their main source.

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LANGUAGE AND EVOLUTION

Summary

Greenberg's conception of the evolution of language as part of the evolution of communication is discussed. A case is made out for accepting the process of gradual orderly change as the central concept of evolution in extra-biological fields, and for regarding the range and complexity of the cultural environment in which a language functions as a yardstick for assessing the stage of evolutionary advance of that language. It is pointed out further that in such evolution, a language undergoes a process of adaptation to the cultural environment which may be viewed as analogous to the adaptation of a biological species to its physical environment.

In a recent essay ¹⁾, J. H. Greenberg has discussed ideas associated with the concept of evolution, and the extent to which language may be regarded as a product of an evolutionary process ²⁾.

Greenberg begins by considering the various uses of the term *evolution* and seeking the common elements in its significance. He rejects as insufficient the simpler meanings of "orderly change" and "gradual change", but accepts as a central conception that of the development of a variety of forms, "whether biological species, languages, or cultural systems", by some dynamic process of growth. In addition, however, Greenberg feels that the further idea of advance of some kind must be added to the conception of change by growth to give the most generally accepted sense of the term *evolution*, and following Herrick ³⁾ who, for biology, takes such advance to be "change in the direction of and increase in the range and variety of adjustments to environment", rejects complexity of form as a criterion of advance, and accepts in its place overall efficiency with regard to the basic

¹⁾ J. H. Greenberg, *Language and Evolutionary Theory*, Chapter 5 of *Essays in Linguistics*, University of Chicago Press, 1957 (= Viking Fund Publications in Anthropology No. 24).

²⁾ Greenberg's discussion is from quite a different viewpoint from that of M. Leroy, *Sur le concept d'évolution en linguistique*, *Revue de l'Institut de Sociologie* Université Libre de Bruxelles, 1949, pp. 337-375), which surveys the conception, within linguistics, of the nature of language development, and the changes in this conception over the last two centuries.

³⁾ C. J. Herrick, *Progressive Evolution*, *Science* 104 (1946), p. 469.

function of language, namely communication in a wide sense. "This leads us to place language in the total frame of the evolution of means of communication. The question of the evolution of language refers to the place of language among other means of communication and whether, in this wider context, a line of evolutionary advance can be discovered".

Such a line of advance Greenberg sees in the development from *prelanguage*, the method of communication by signs or gestures, or perhaps even symbols, but lacking systematic combinations or constructions of these elements, to *language*, which is characterised by the development of grammatical system, and which presumably first appeared with hominids, and further through to *postlanguage*. By the latter term Greenberg refers to writing and code systems based on language, new sign systems such as those of mathematics and symbolic logic, and artificial languages. "The development of forms isomorphic to language, . . . and the appearance of postulated sign systems which overcome in good part the semantic and grammatical inefficiencies of language . . . may be interpreted as a process of differentiation and specialization within the communication process, whereby each communication need becomes more efficiently served by an instrument which more adequately fulfils some specific function".

His general conclusion "then, is that it is not language as such which evolves but rather communication in general. Within this process language does have a central and key position as the source of all postlanguage developments and the general instrument which fulfils the function of the ultimate level of explanation. While it may seem somewhat rash to prejudge the case, it appears that natural languages are all very much on the same level as far as efficiency is concerned. . ."

Greenberg's discussion is an interesting one from this point of view, but the reader is likely to leave it with a feeling of disappointment, a feeling that Greenberg has failed to give adequate consideration to what would seem to most linguists a more immediate question, namely, whether a language can be said to evolve. That any spoken language is continually subject to development is generally accepted, but the relationship between such development and the processes of evolution in biology have been, perhaps understandably, rather neglected over the last few decades. It seems worthwhile then, to take

Greenberg's essay as a starting point, and by modifying somewhat his theoretical framework, to discuss this problem to some purpose.

The first of the two ideas which Greenberg takes to be essential in the semantic field centred round the term *evolution* is that of the origin of a variety of forms from some common ancestor by a process of dynamic growth. In this sense, he points out that the general acceptance of a process of evolution was earlier in linguistics than in biology – it was first suggested, as is well known, by Sir William Jones in his famous address to the Asiatic Society in Calcutta in 1786, and was generally accepted by the middle of the succeeding century. There have been, since then, a few attempts to reject this conception of development in language but none have made any serious breach in the general consensus of opinion that languages may evolve in this way.

Nevertheless, it may be doubted whether this conception of differentiation of an earlier unity into a number of later unities is by any means central to the idea of evolution. In biology proper, such development, which is most clearly reflected in speciation, the development of diverging lines of descent from a single group of ancestors, is termed adaptive radiation; this is part of evolution in general, but only part, and is dependent on a more fundamental concept, that of the possibility of change in the species. The mechanism by which this possibility is established, the existence of individual variation, and so on, are beyond our concern here; what is important is the recognition of the possibility of the series of successive changes over a period by which a species may become modified. The biologist legitimately says that a species evolves without referring to anything more than this type of bodily or behavioural modification by which succeeding generations of a species come to differ from their ancestors. This sense is probably the most usual one in extra-biological fields; we can speak of the evolution of the typewriter, for example, referring to the series of changes which have occurred over the years in the form, structure and operation of the "species" typewriter, without necessarily including its diversification into portable typewriters, electric typewriters, mathematics typewriters, and so on; and in the same way we can speak of the evolution of the idea of democracy, or of coinage, or of any other cultural concept or institution.

In this sense also, it would seem perfectly proper to refer to the

evolution of a language. The data from the historical study of all languages in which such study has been undertaken is quite unequivocal: a language in its natural spoken form is continually subject to change through time. The rate of such change may vary from time to time, as may that portion or part of the language in which change occurs, but the fact of such change, encompassing, given sufficient time, all the aspects of a language, its phonology, grammar, and lexicon, is now as well established as any other fact in linguistics. The successive modification of the totality of a language which results from such change may well be viewed as the evolution of that language.

The second of the two ideas which Greenberg associates with evolution is that of advance or progress of some kind, and it is surely worthwhile to consider in what way, if any, the evolution of a language in the simple sense of gradual or orderly change through time leads to any such advance or progress in that language. He quotes approvingly from Herrick (as above), and points out that increase in the discriminatory and functional capacities of both receptor and effector organs, and in the nervous system and its operating processes are all developments which may be regarded as exemplifying evolutionary advance. Of two species, one may be more advanced in some respects and less advanced in others, but nevertheless, on an overall basis, some species are in general more advanced than others, and it can fairly be said that the less advanced species have appeared earlier in time than the more advanced. At this point in his discussion, Greenberg, instead of following up this conception and considering language in relation to it, veers aside to introduce a new idea, that of efficiency. He relates this idea to the basic function of language, namely communication in society, and concludes that there is apparently very little difference between natural spoken languages with regard to efficiency. But efficiency is difficult to assess, as Greenberg notes; languages, like animals, must be efficient to an acceptable degree, in their own environments.

Let us leave aside this question of efficiency and return to the consideration of evolutionary advance, in the biological sense. Increase in the functional diversity and efficiency of the bodily structures of a species not only enables that species to make finer discriminations in perception and more varied response to the environment, it also, in a very real sense, increases the ability of that species to adjust to

environments of different or greater range, size and complexity. Amoeba and other relatively simple organisms, in which only the most rudimentary sensory and motor organs exist, must be limited to a very small range of different environments of comparatively simple and uniform types. In a more advanced species, such as a bird, the ability to cope with more complex environments is immeasurably greater. In the case of man, this process has advanced to the stage that of the whole land surface of the globe only the highest regions of the mountains and the most extreme areas of the arctic and antarctic are beyond his physical capacity to live in. And moreover, man, even the most primitive, has, by his own efforts, erected a new type of environment, that of culture, which supplements the physical environment and thus increases beyond anything found among non-human species the complexity of the total environment in which he lives and to which he reacts. Biological advance, then, in the physical structure of a species is also reflected in the environment in which that species lives: the more advanced the species, the more complex the environment it inhabits tends to be.

This state of affairs seems to hold in the vast majority of cases, and it should be possible therefore to use it in reverse. If the extent or complexity of the environments of two species could be assessed, the assay should give an indication of the relative state of evolutionary advance of these two species. Such assessments are obviously difficult to make as yet in any precise fashion, but nevertheless, in a general way, as above, they seem to be possible.

With these considerations in mind, we may turn to the question of language. A language does not exist in vacuo; it functions in a particular environment, and this environment must surely be the culture of the community using it. The members of a community use their language to communicate with one another, but the information communicated lies within the culture of their community. All communications, save perhaps in the marginal cases of initial introduction or invention, must refer to entities or relations which are overtly recognized in the culture. The function of the language of a particular community is clearly then to refer to, to relate and generally to deal with all those topics within the culture of that community which the members wish to communicate.

It is obvious that the cultures and hence the environments in which

languages are used vary very considerably. The complexity of the culture of a highly industrialised, socially stratified community with a wide range of practical and theoretical interests is clearly greater than that of a socially homogeneous community depending on say food-gathering and elementary agriculture. Between the extremes of this sort comes a whole gamut of cultures varying in diversity and total complexity. Now languages may be well adapted to the particular cultures in which they are used, and may all, as Greenberg suggests, be more or less equally efficient in those contexts, but it is obvious that the range of topics which the language of a very complex culture is required to cope with is very much greater than that required from the language used in a relatively simple culture.

A community with a complex culture such as is exemplified in a modern industrial civilisation requires from its language not only means of referring to all the multifarious objects and ideas which exist in that civilization, and to all the relations and connexions which it makes between them, but also relies heavily on its language for communication within and between the subgroupings and the strata of that civilization, and for the organisation and functioning of the great cultural institutions, commerce, science, industry, etc. which are characteristic of such a civilization. In comparison with this, a community such as the Eskimo, whose material culture is restricted and whose social organisation is largely limited to the family group will have, simply, much less to talk about, and consequently make less extensive and less exacting demands on its language.

It would seem then that we are justified in concluding that the environment within which a particular language functions may be of very much greater range and much more diversified than that of another. But such environments are of course not static. The culture of a community is itself subject to change from period to period. Because of the basically cumulative nature of human culture, it is probable, as Kroeber ⁴⁾ points out, that the predominant and most usual trend is from a simpler state, i.e. one in which the community has relatively few activities and interests, to a more complex one, i.e. one in which the community has a relatively large number of such. The reverse trend may indeed also occur, though less frequently, and even in a

⁴⁾ A. L. Kroeber, *Anthropology* (New York, 1948) p. 297.

culture in which the development is predominantly toward complexity, simplifications and loss may well occur in restricted fields.

The more complex cultures have thus arisen from a process of incremental development of simpler cultures. In this argument, it makes no difference whether the increments in the process are of internal development or of external borrowing. Consequently, the language functioning within the environment formed by such a culture, assuming of course continuity of it, has not only evolved in the simple sense of having undergone gradual or orderly modification through successive periods of time, but has also changed in such a manner as to maintain its ability to cope with the increasing demands of communication made upon it by the members of the community. As the environment in which it is used has come to contain more and more objects, institutions, ideas and relations, so the language has developed to cope with the handling of them.

Now it would seem not unreasonable to view such a development not simply as a case of evolution in language, but as evolution towards a more advanced stage. The language which has evolved to the stage where it is able to cope with the demands of communication over the wide range of topics which characterize a complex civilisation may legitimately be regarded as having developed to a higher stage than a language which has not so evolved. In terms of the biological analogy, we may say that a language which has become adapted to the communicatory needs of a group with a complex culture functions within a wider and more differentiated environment, and by so doing reflects a more advanced stage of development.

An inference of this nature, I suspect, will tend to be rejected by linguists, to whom it seems almost an article of faith that any language is as good as any other, and to whom any sort of judgment as to the relative values of languages is unscientific. This attitude is doubtless partly a result of the linguist's concentration on the formal side of language. It is admittedly difficult to see any real reason why 20 phonemes in a language should be classed as better or worse than 30 phonemes, or whether the expression of a specific grammatical relationship by internal vowel change, should be preferred to its expression by suffixation, or to its expression by the addition of a separate lexical unit. But when the whole of language is considered, and in relation to the culture in which it is used, the position is

somewhat different. The assumption that all languages are more or less equally efficient in their respective cultures may be retained, but at the same time the differences in these cultures may be used as an external yardstick to assess the amount and the complexity of the work which languages are called upon and are able to perform.

It may also be noted that the differences in the environments of languages are not limited to quantitative-like differences of complexity and extent. One of the main contributions of the study of cultural anthropology to modern understanding has been the realisation of the qualitative differences between cultures, in the sense that somewhat differing aspects or features of the external or perceptual world are selected in different cultures, and that these aspects may be patterned and organized in different configurations. Not only indeed may items which are overtly recognized in one culture differ from those in another, but even where universal features of human life and experience are concerned – birth and death, marriage and kinship, etc. – these may be integrated into the total culture pattern in varying ways, and reacted to quite differently.

In this qualitative differentiation of cultures, language is an important factor. A language is of course an integral part of a culture and thus contributes, at any point in time, to the specific character of that culture and to its difference from all other cultures. But a language also functions within a culture, referring to, in so far as they are referred to, the objects, ideas, activities, processes, relations, and so on which go to make up that culture.

The realisation of differences in the ways in which languages categorize and relate the aspects of the perceptual world which a community selects and incorporates in its culture is largely due to Sapir and Whorf. Though no extreme view as to the influence of a language on the *Weltanschauung* of a community need be accepted, it appears probable that this categorization and relation of the features of a culture is at least partly a function – in a quasi-mathematical sense – of the total lexical and grammatical structure of the language concerned.

From the viewpoint adopted here, this type of relationship between a language and its culture may also be regarded as one of adaptation: in much the same way as languages are adapted to their functioning in cultures of differing total complexity, so also are they adapted to their

functioning within cultures of differing qualitative nature. If such and such features of community experience are overtly recognized in a culture, then the language associated with that culture has the means of referring to and relating them.

This also seems to have something of a parallel in biology. It has become apparent that the sensory apparatus of a species is adapted to and responds to aspects of the world environment which may be different as a whole from those reacted to by the sensory apparatus of another species. There is evidence, for example, that the bee is sensitive to the plane of polarised light, that the bat reacts to a range of sound-wave frequencies which is far beyond that of any other mammal, that certain fishes react to changes in electrical field, and so on. There is here thus a selection of varying aspects of the external world by different species which is perhaps analogous to the selection of varying aspects of the external world reflected in the structure of different languages. Just as a species, as a result of the specific structure and operation of its sensory apparatus must react to and select an environment for itself, so also a language, as a result of its grammatical and lexical structure, will operate only in an environment to which it has reference and will tend to be inappropriate for communications dealing with aspects of the world which fall outside this environment.

The adaptation of a language to its culture and to developmental changes in that culture is likely to be taken at first sight to be an adaptation with regard to the lexicon of the language concerned. "It goes without saying that the mere content of language is intimately related to culture. A society that has no knowledge of theosophy need have no name for it; . . . In the sense that the vocabulary of a language more or less faithfully reflects the culture whose purposes it serves it is perfectly true that the history of language and the history of culture move along parallel lines. But this superficial and extraneous kind of parallelism is of no real interest to the linguist except in so far as the growth or borrowing of new words incidentally throws light on the formal trends of the language".⁵

It is probable however, that this is rather a restrictive view which is unlikely to command full assent today.

The evolution of a language through time is a process affecting all

⁵) E. Sapir, *Language* (New York, 1921) p. 234.

its subsystems, and developments in one subsystem are likely to have reverberations through the whole system. The loss of a phonemic opposition, for example, may affect the valency of some inflexional morpheme, and this in its turn may lead to a change in a syntactical pattern. This is clearly illustrated in the history of English over the last few centuries, where the reduction of stress in final syllables has led to weakening and loss of a great deal of the inflexional system of Old English, and thence to the development of the more analytical patterns of expression which are characteristic of the present-day language. This development has had effects on the vocabulary too, mainly, doubtless, in the form of semantic changes which have overtaken the numerous words acquiring grammatical function at the expense of lexical function – the auxiliary verbs, *shall*, *will*, *can*, *may*, *have*, *do*, etc., and the prepositions, *of*, *in*, *through*, etc., for instance, and in the loss of a large number of inflected forms of words – but partly also in the development of a few words – the articles *a*, *the*, and the gerunds – which did not exist in the earlier stage of the language.

The direction of development in this example seems to be primarily from the phonological level through morphology and syntax to the vocabulary. In other cases, the direction of such development seems to be the reverse, a cultural change leading to change in the grammatical or phonological system of the language concerned.

One of the striking changes in England over the last couple of centuries has been the urbanisation of the population. A minor result of this has been the change in the nature of the immediate environment of a considerable proportion of the speakers of the language and of course of the child during the period of his language learning, particularly with regard to the common farm animals. Gradually over this period, the strong preterite forms *crew* and *shore* have become obsolescent, being replaced by *crowed* and *sheared*. The strong verbs *crow* and *shear* are thus in the process of becoming weak verbs, and it would seem not unreasonable to connect the exposure of these to the processes of analogy at the morphological level with the receding of the experience denoted by them to a less familiar and more derivative state. A further example from similar causes is probably the current disappearing of the last simple weak plural in English; today *oxen* survives, we may be sure, rather because of its nature as an exception and

consequent incorporation into grammatical education in the schools than because of its use in the living language. To trace further effects, through to the phonology is more difficult, though it is easy to appreciate the theoretical possibility of a series of such changes affecting the phonemic structure of a language with regard to the combinations and patterns of phonemes which occur. We know this may happen, of course, in the case of cultural effects resulting in the introduction of loanwords, as lexical borrowing from Greek has added the sequence /sf-/ – *sphere*, *sphinx*, *sphincter*, – to the inventory of initial clusters in English.

Save in the exceptional circumstances of very close and long-lasting linguistic contact, cultural changes with effects spreading in this direction through the different levels of a language are generally few and relatively minor, but the examples above nevertheless suggest that, given time, they may well lead to quite substantial alterations in the grammar and phonology of a language.

From the preceding, it is clear firstly that the fact of change in a language over a period of time is well established, and it would appear that in this change a language undergoes a process of development which is analogous to the process to which the term *evolution* is applied in the biological field, and homologous to the process of development to which the same term is applied in extra-biological fields, and it would appear unnecessarily pedantic to deny the use of the term to language development. A language thus may legitimately be said to evolve.

Further, however, the evolution of a language shows or at times may show two distinct characteristics, both of which have analogues in the process of biological evolution. The first is that in certain circumstances, the evolution of a language may be such as to enable it to function or to continue to function as an efficient means of communication within a culture which is becoming more extensive and more complex. This may fairly be described as a process of evolution toward a more advanced state; the language comes to function within an environment of wider range, as, analogously, a species in its evolution may come to respond to or function within an environment of greater range and complexity.

The second is that in its evolution a language may come to function within an environment which, though not perhaps differing in total

complexity from that of another language, yet differs in that it is specific to that language, extending over a range which does not coincide with that of any other language. This seems to be analogous to the process in evolution whereby a species comes to inhabit a specific ecological niche, a segment out of the total range of the environment of living things.

None of these conclusions, it must be noted, imply that all changes which take place in a language over a particular period are necessarily contributory to the processes of advance and adaptation; some changes may be quite neutral to these processes, and some, in theory at least, may lead to results reversing or tending to reverse them.

Nevertheless, given the generally cumulative nature of the development of a culture, and the maintenance of a similar degree of adequacy and efficiency of a language, it would seem that the bulk of the changes in a language over a period do contribute to the processes of advance and adaptation which seem to be inseparable from the evolution of a language.

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STRUCTURALISM AND RECONSTRUCTION

Some notes on the verbal system of Primitive Germanic

Summary

In this article, an attempt is made at a purely synchronic description of the Primitive Germanic verbs, in which the use of such diachronically coloured terms as 'preterite-present' and 'aorist-present' is avoided. The term 'ablaut' is made to fit into the synchronic system. The term 'quantitative ablaut' is only used when, within one series of categories, there is a functional opposition between two phonemes and one phoneme. The author argues that the phonic structure of the roots must not be taken as a criterion for classification, and that the starting-points for a classification of the strong non-reduplicating verbs should be the various series of categories and the relations between their members. He further puts the case that a system is not primarily defined by what occurs most frequently, and that the residual groups, too, should from the outset be included in the system.

1. *Introduction*

Structuralism does not restrict itself to the description of living languages. As appears from recent historical grammars and revised editions of old ones, and as appears from a number of separate studies, it is beginning to feel at home in the exploration field of historical and comparative linguistics. In the first place, the concept of the phonological system is being made use of for the explanation of certain sound laws. In the second place, the view that language is a system is being made to underlie the description of earlier linguistic phases. And to these linguistic phases also belong such as have not been recorded in writing, but have been reconstructed after a historical and comparative investigation.

Where the systematics of non-empiric data, i.e. the structure of reconstruction is concerned, the linguist will have to be aware, more so than in other cases, of the dangers that beset him. As it is, a preconceived reality is less opposed to aprioristic thinking than is something of which the existence cannot be doubted, and which appears regularly and can be observed in all its complexity. The characteristics of the method he is using, and the peculiar nature of his object of investigation, compel the structuralist applying himself to such recon-

structions as Primitive Germanic or Primitive Indo-European, to keep the following points continually in mind.

1. "Language is a system" can only be regarded as a working hypothesis. Never should facts be neglected for the sake of a greater unity in the description, and never should anything that has not been proved to be a fact, or has not at least been made acceptable, be put forward as a fact.

2. 'Discovering' the system of a complicated phenomenon can never be anything else than grouping together on the basis of similarities, and differentiating on the basis of dissimilarities. Is it always clear why some similarities or dissimilarities are considered relevant, others irrelevant, some primary and others secondary?

3. The terminology needed for grouping together or differentiating should be unequivocal and not, for instance, at the same time diachronic and synchronic, or phonetic and phonological.

4. A language like Primitive Germanic is not one language in the strictly synchronic sense, but a group of languages or linguistic phases, one succeeding the other in time. When describing the verbal system of a particular period of Primitive Germanic, one should ascertain if the temporal relation between two sound laws is not implicitly put forward as definite when it cannot be or has not (yet) been proved.

2. *The weak verbs*

It can hardly be maintained that, before structuralism came into being, the linguistic approach to the description of Germanic verbs was completely unsystematic. Objections can be raised to the terminology used, and also to some of the classifications. But there cannot be any difference of opinion as to the relevance of the similarities and dissimilarities on which most of the divisions and subdivisions are based. Thus, the primary differentiation between weak and strong verbs is not essentially affected by the new views. These terms do not seem to be hazardous, they can be retained. They are, it is true, originally metaphors, but these have completely faded and are therefore absolutely harmless.

It is clear on what principles the above differentiation is based. It is on the way in which the preterite and the past participle are formed, namely with or without a dental suffix. If we take this privative opposition – the presence or absence of only one characteristic –

seriously, then the so-called preterite-presents, too, should be included in the former group. In some handbooks of Primitive Germanic they are in fact mentioned together with the weak verbs, in others they are given as a separate group by the side of or in opposition to, the strong and the weak verbs. But whether they are considered as a primary or as a secondary group, they are always referred to by a name that is out of place in a synchronic description of a language.

The following is the most usual definition: "Preterite-presents are verbs which combine the form of a strong preterite with the meaning of a present." And indeed, when we compare Gothic *kann* – *kunnum* with *rann* – *runnum*, it seems difficult to come to any other conclusion. And yet, *kann* – *kunnum* are present-forms of a weak verb, which are in opposition to preterite-forms (of the same verb) with a dental suffix. So, two opposite statements are made about one and the same empiric datum. This is only possible if in both statements a word occurs that does not retain its identity and which is used differently in either case. Those who speak of a preterite-form in the case of *kann* mean something else by 'form' than those to whom it is, (just as) formally, a present. But establishing this is not enough. The question remains as to which of the two handle the word correctly, that is, in a way conforming to the object of investigation.

There is a Greek verb *χάσκω*, 'to open the mouth', with the forms *χανοῦμαι*, *ἔχανον*, *κέχηνα*. We are told that the perfect means 'to yawn'. But that is too simple a representation of matters. The Greek word *κέχηνα* has a more intricate semantic structure than English 'I yawn'. We cannot just say that in this case a perfect-form is combined with a present meaning. For the latter, not having a perfect, is not opposed to a preterite meaning, and therefore, 'present' here means something else than in cases like "*I yawn* is a present". In the second place, our 'perfect-form' is not a form only, but a form with a meaning, the perfective meaning. The semantic implications here are, though not the same as, comparable with those in the case of the metaphorical use of a word: the word retains its meaning even though it refers to something different. Our form can even better be compared with what is called a 'historic present'. This is no doubt not only a present formally (whatever that might mean), its stylistic effect is based on the fact that it also has the meaning of a present, while it is used for an action in the past. Thus, *κέχηνα* has the meaning of a perfect even now

that an incompleted action belonging to the present is indicated by it. It has two 'meanings' at the same time, but it does not have them in the same way. Only in such a situation can the term 'perfect-present' or 'preterite-present' be used in a synchronic description.

In the Germanic languages, however, a so-called preterite-present is not the preterite of an existing verb in those languages. And it has a present because it has a preterite. If we ascribe to such a verb a preterite-form in the present, we abstract from the meanings, forgetting that where language is concerned, 'form' and 'meaning' are correlative notions. Now, could we say that we are then formulating diachronically, that we are etymologizing? Yes and no. Some of the verbs included in this group once had the characteristics mentioned in the preceding paragraph. For others this either does not hold good or it has not been proved, so that the objectionable term is even less applicable in their case.

As to the former group, there is a persistent misunderstanding – the same that was responsible for the term 'etymology' to refer to one branch of historical linguistics. 'Origin' and 'essence' are, wrongly, identified. It is incorrect to attribute to a word in a certain language characteristics which it had originally, in its earliest-known phase, i.e. when it was a *different* word in a *different* language. The simple truth that a word is not what it was then but what it is now, has not yet fully penetrated. It is not only in handbooks of Primitive Germanic and in descriptions of Old Germanic languages, but also in historical grammars of younger languages, even in grammars of present-day English, that a number of verbs are referred to as 'preterite-presents'. And so, several words have carried along – in linguistics, not in language itself – the hereditary burden of their Early Indo-European origin, until close on 2000 A.D. The idea of 'form' referred to above, lacking as it does any functional relation, is the result of this historicizing view.

Meanwhile, this criticism has not simplified matters. Or rather, it has shown them up in their complexity. The structuralist is now saddled with a group that does not, for the time being, have any acceptable name, and one whose characteristics in connection with the others verbs have not yet been described. If he wants to define them by means of a comparison with other verbs, as has been usual so far, all he can say is "There is a group of weak verbs of which the forms of the present are the same as the preterites of certain strong verbs,

provided we disregard the undeniable fact that the former are in functional opposition to preterite-forms, as are the latter with regard to present-forms". This definition is far from satisfactory. It compels him to describe all the other weak verbs as 'verbs of which the present forms are *not* the same as . . . ' etc. Besides, there is another mistake in the usual conception that he has not yet eliminated in this way. For not all the verbs that are called 'preterite-presents' have the characteristics mentioned in the definition. Thus, it is in vain that we shall look for an analogue of Gothic *mag* – *magum* among the preterites of strong verbs. The same may be assumed of its Primitive Germanic ancestor. Its etymology makes it unlikely that Gothic *magum* should have been formed on the analogy of the singular.

For these reasons we may well ask if we had not better refrain from any comparison with the strong verbs, however plausible it may appear. But then what can we do? May not these verbs have always been incorrectly grouped together?

There are at most fourteen or fifteen of them. We must further supplement the Gothic material with the Old Norse, and also reckon with the West Germanic. It is not certain that all of them have belonged to Primitive Germanic, and not enough forms have been handed down to make a conclusion as to their characteristics in the reconstructed language possible. We may take it that ten of the Primitive Germanic verbs had a (root) vowel alternation in the present, on which (among other things) the distinction between singular and plural was based. This phenomenon may be called internal ablaut. As opposed to the vowel alternation within one tense, there is the external ablaut, e.g. in Gothic *niman* – *nam*. The term 'ablaut' should here be taken strictly synchronically. The implications of this I hope to deal with when I have come to the strong verbs.

The weak verbs then, may be divided into two groups: those with and those without internal ablaut in the present. This is undoubtedly a relevant distinction. Moreover, the criterion applied has the advantage that it can also be used for subdividing the strong verbs, which makes for greater consistency. As far as we can make out, this internal ablaut is accompanied by a (partial) external ablaut (e.g. in Gothic *kann* – *kunþa*). And this would induce us to include in our classification the verb which is *briggan* in Gothic. But in that case it will be preferable to consider the contrast 'ablaut – no ablaut' first

and only then to apply the internal ablaut criterion. This again is in accordance with what we find in the case of the strong verbs.

Starting from these considerations we may provisionally picture the system of the weak verbs as follows:

TABLE 1

Verbs with dental suffix in preterite and past participle

with ablaut		without ablaut
with internal ablaut	without internal ablaut	

Against this it might be adduced that here a primary classification is based on the existence of one single verb. The question remains, however, as to how many similar instances there should be of something for them not to be excluded as deviations or irregularities from any systematic connection. Any number given here cannot, I should say, but be most arbitrary. If a category is represented by only one case, or a few cases, that should of course be stated. But the idea that the 'real' system should be determined by what occurs most frequently, has unacceptable consequences. However, the rejection of the quantitative principle, too, has its drawbacks, certainly in our case.

The classification we have suggested, namely, is not satisfactory in the case of some verbs. It has no doubt its advantages. Thus, with regard to the so-called preterite-presents of the fourth class (Gothic *man* – *munuþ*, *skal* – *skulum*), it is no longer necessary to say that they differ from the preterites of the strong verbs of that class in their zero grade in the plural; as it is, they conform to the criterion of the internal ablaut, which is only concerned with a difference between vowels and not with the nature of these vowels. But what can we do with the verbs whose first person singular in Gothic is *mag*, *ga-mot* and *og* respectively? According to the usual ideas, the last two are not 'deviations', as they correspond with the preterites of the strong verbs of the sixth class in every respect. If we stick to the classification suggested, all three should be included in the group 'weak verbs without ablaut'. The difference between them and the Gothic verbs that are known as *jan-*, *on-*, *an-* and *nan-*verbs can then be discussed when we come to sub-dividing the group mentioned. In this way, they are completely separated from the weak ablaut verbs. Yet, they have

something in common with them that can hardly be said to be unimportant. It is their personal endings. In Gothic these are (not including the dual forms): $-, t, -, (u)m, (u)\bar{p}, (u)n$; or rather: we may assume that that is what they were in Gothic. On the other hand we find, in the four old-established classes: Gothic a or $o, s, \bar{p}, m, \bar{p}, nd$. If we try to render that in Primitive Germanic, we get essentially the same picture: the weak verbs with internal ablaut and the three verbs mentioned above differ, in the present tense endings, from all the other weak verbs in many respects.

We have, then, two different characteristics, though both relating to the conjugation. One concerns the root (vowel), and gives us two groups: $a1$ and $a2$. The other concerns the endings, and also gives us two groups: $b1$ and $b2$. Theoretically, there could be four groups; in reality there are only three, namely $a1$ plus $b1$, $a2$ plus $b1$ and $a2$ plus $b2$. There are no weak verbs with internal ablaut and personal endings of the second type. As we shall see presently, we find the same thing – two criteria, three groups – in the case of the strong verbs. In such cases it is only on the strength of all the data, that we can determine which of the two criteria should have priority. As the strong verbs cannot be classified on the basis of b – they do not differ from each other as regards personal endings – we had better, obviously, use a rather than b for a classification of the weak verbs. Moreover, this is in accordance with the fact that the further we subdivide, the greater the degree of concreteness becomes.

By making use of concrete endings we have discarded the formula of systematization 'presence/absence of a characteristic'; $b1$ and $b2$ can only be described positively. Nor can the formula be used in a classification of the weak verbs with the endings of the second type. These verbs differ from each other in what immediately precedes the ending. The difference between Gothic *sandjan*, *salbon*, *haban* and *fullnan* cannot be described but by placing their present tense paradigms side by side. Working with privative oppositions has its inevitable limits. I do not think I overstep these limits by suggesting the following classification of the weak verbs on the basis of my above considerations:

TABLE 2

Verbs with dental suffix in preterite and past participle

with ablaut		without ablaut				
with internal ablaut (present endings) type I	without internal ablaut type II	type I	type II			
			1	2	3	4

Perhaps not everybody will accept this suggestion. Personally, I think that an analysis of the concept of 'ablaut' will render alterations desirable if not necessary (see below). One thing is certain: the term 'preterite-present' will have to disappear from the descriptions of the Germanic languages and of Primitive Germanic.

3. *The strong verbs*

So far, we have been able to evade almost entirely, the difficulties of reconstruction, and we have had no need to speak of the chronology of the sound laws. For a systematic descriptions of the strong verbs, we shall appear to need the aid of historical and comparative linguistics much more than before.

The strong verbs have been characterized only negatively in their contrast with the weak ones: verbs without a dental suffix in the preterite and past participle. As not all of them form their preterite positively in the same way, the same principle can be used for a subdivision as the one on which the distinction 'weak - strong' is based. This subdivision, too, is already an old one. It has been usual to oppose the ablaut verbs to the reduplicating ones, and to divide the latter into reduplicating and reduplicating-ablaut verbs. This is satisfactory neither from a terminological nor from a systematical point of view. 'Reduplicating' has to denote both a genus and a species at the same time. Further, the term 'reduplicating-ablaut' would suggest that 'having ablaut' and 'reduplicating' are not contrasting, while in the primary distinction they do appear as contrasts. Here again, we have two oppositions, namely 'with ablaut - without ablaut' and 'reduplicating - not reduplicating'. There are, however, only three groups. Once more we are faced with the questions as to which criterion is primary and which secondary. With the weak verbs, reduplication does not, ablaut does occur. Therefore, it is desirable to consider the

latter as primary. The classification, in which we have, for the time being, to restrict ourselves to kinds of the first and second degrees, will then be as follows:

TABLE 3

Verbs without a dental suffix in preterite and past participle

with ablaut		without ablaut
with reduplication	without reduplication	with reduplication

I shall further confine myself to the verbs with (external) root vowel alternation without reduplication. All these verbs have in common, besides two negative characteristics, that the root vowel of the present does not occur in the preterite. This does not tell us anything about the nature of the vowels, nor about other phonic characteristics of the roots. How is this large group to be subdivided?

To this question an answer was supplied long ago. The group consists of six classes, which break up into two groups: the first five as opposed to the sixth. In his interesting study *Das System der starken Verba und die Periodisierung im älteren Germanischen*¹⁾, Fr. van Coetsem speaks of the e-group, as a comprehensive term for the first five, which he characterizes as follows:

TABLE 4

1. e + i + consonant
2. e + u + consonant
3. e + liquid / nasal + consonant or e + double liquid / double nasal + consonant, sometimes double consonant
4. e + liquid / nasal
5. e + consonant

Clearly, an essentially different criterion is applied here from the one we have so far used for the various classifications. 'Without dental suffix', 'with ablaut', 'without reduplication': successively these three characterizations tell us something about the way in which the preterite is formed; together they characterize the preterite-form of the verbs

¹⁾ Mededelingen der Koninklijke Nederlandse Akademie van Wetenschappen, Afd. Letterkunde. Nieuwe Reeks, Deel 19, No. 1. — N.V. Noord-Hollandsche Uitgevers Maatschappij, Amsterdam, 1956.

in question, in its contrast with that of other verbs. It would seem reasonable to stick to this or a similar criterion instead of switching over to another, which has not really any connection with it: that of the phonic structure of (part of) the root, especially (as regards the vowel) of the present-root.

For the sake of consistency we shall have to ask ourselves: is something of this kind also to be found in the classification of the weak verbs? Is there, too, the nature of the consonant(s) of the root taken into account, as well as that which is, within the conjugation of one verb, formally invariable and therefore non-functional?

This is indeed the case. The Gothic *jan*-verbs are divided into two groups, those with long and those with short roots. In table 2 this aspect was not taken into account. When we compare the Germanic languages among themselves, we find that this division was not yet completed in Primitive Germanic. But in Primitive Germanic, too there must have been a difference between long and short roots, judging from the strong verbs with *ǣ* – *īo* – suffix in the present. As this distinction goes right across the contrast 'weak – strong', I prefer to compare the classification of the Gothic weak *jan*-verbs with that of the strong verbs according to table 4.

As to the former, we can argue as follows. The quantity of the root syllable would be a completely irrelevant criterion if these verbs did not at the same time differ from each other in another respect. Gothic *sandjan* and *nasjan* differ from each other in the second syllables of three finite forms of the present. This difference is primary as compared with the other. If the whole group had consisted of these two verbs only, a division into two would still have been necessary. But from a synchronic point of view we should not have had the right to speak of long and short roots unless we reckoned with what we find in the nominal *ja*-stems. We could just as well have said that one begins with *s* and the other with *n*. Now that the group contains many verbs, we are able to say that all the verbs characterized by a second and third person with *-ei-* have a long root, and that all the *ji*-verbs have a short root. In the descriptions, however, exactly the opposite is always found: "the verbs with long roots have one, those with short roots another paradigm". What is secondary is made primary there, and different similarities and dissimilarities are used as criteria from the ones that have so far been used as a basis for classification. In the

discussion of the weak verbs of the second class the quantity of the root syllable is, rightly, neglected, though *salb(on)* is not shorter than *sand(jan)*, and *kar(on)* not longer than *nas(jan)*: in the conjugation there is not really any difference between *salbon* and *karon*. Do we need more evidence for the existence of the hierarchy alluded to?

The same process of reasoning is to hold good for the strong ablaut verbs without reduplication. The system of conjugation of each of them is characterized by four forms: the first person singular of the present, the first person singular of the preterite, the first person plural of the preterite, and the past participle. In the first instance all we can do is investigate how many series of four, differing from one another in one or more respects, there are. After that, we can group all the verbs having the same series, together. Only then shall we be able to find out if the members of such a group conform in their root forms.

However, we meet here with a peculiar difficulty. Let us not forget that we are trying to discover the verbal system of a reconstructed language. *Sandjan* and *nasjan* have come down to us with their conjugations; but of Primitive Germanic, nothing has come down to us. And now it appears that we cannot be certain of the categories as long as we have not first reconstructed the root forms. Now, if Primitive Germanic is not one language but a group of languages or linguistic phases, one succeeding the other in time, we should first make up our minds as to which period we wish to reconstruct something for.

This question Van Coetsem also asked himself, in his above-mentioned study, and he also supplied an answer. Table 4 refers to a linguistic phase which he calls the 'e- a- period', and which he characterizes with the help of the temporal relation between two sound laws: Indo-European *a* and *o* had already been levelled under *a*, but the conditioned sound laws had not yet affected the *e*. He succeeds in making it very acceptable that there has been such a period and that Primitive Germanic then had a 'four vowel system' (short: *i*, *e*, *a*, *u*, long: *ī*, *ē*, *ō*, *ū*). Connected with this is his other thesis: the ancestors of Gothic *beitan* – *bait* and *giutan* – *gaut* had, not one but a group of two phonemes between the two consonants, namely *e + i̇*, *a + i̇*, *e + u̇*, *a + u̇*. I fully agree with this thesis. Now that we are certain of the root forms, we can reconstruct the series of categories: 1. *e + i̇* — *a + i̇* — *i* — *i̇*,

2. $e + \bar{u} \rightarrow a + \bar{u} \rightarrow u \rightarrow u$. And not: 1. $e\bar{i} \rightarrow a\bar{i} \rightarrow i \rightarrow i$, 2. $eu \rightarrow au \rightarrow u \rightarrow u$, both with four times one phoneme.

For our reconstruction we have to use this method. For a classification, however, we can only proceed as we do with recorded languages. For that matter, the reconstruction is only possible because in Gothic etc., the verbs with a particular series of categories also appear to have had a particular root form.

In table 4, five root forms have simply been coordinated to each other. All these have *e* in the present and *a* in the preterite. If we rejected the theory of the two phonemes, we could say this of only three of them. Van Coetsem does not stop at this coordination. He takes 1, 2 and 3 together as opposed to 4 and 5, like this:

TABLE 5

- I. $e + \bar{i} / \bar{u}$ / liquid / nasal, $e +$ double liquid / double nasal or double consonant;
- II. $e +$ single liquid / single nasal or other consonant.

I should like to make a few objections to this.

1. Forming groups on the basis of the structure of the roots is only useful if all the members of a group have the same type of conjugation. Group I seems to be homogeneous, if we reduce the three classes to: $e \rightarrow a \rightarrow \text{zero} \rightarrow \text{zero}$. As I hope to show presently, this method is partly diachronic and therefore inconsistent, or it is based on a conception of the Primitive Germanic consonant system of which the correctness has not been and cannot, I think, be proved. Group II obviously has two types of conjugation, differing in the fourth category. If we should reduce them to the same denominator on the basis of their agreement in three categories, we really ought to use the same criterion for the systematization of *all* the strong ablaut verbs without reduplication.

2. The term 'double nasal' ('double liquid') is unacceptable, unless it should mean that there are, for example, two successive *n*'s and that two *n*'s are also pronounced successively. If it means 'long', then why are we not told so? What is it that compels linguists to stick to this unfortunate terminology? There is nothing to be said against the use of the technical term 'geminate'. We can mean by it a long consonant, and do not have to attribute to it the etymological meaning, which is based on a misunderstanding (mixing up of sound and symbol). Thus

we still speak of Red 'Indians', although we have known for a long time that these people do not live in India. But it is ridiculous to say 'red inhabitants of India' instead of 'Red Indians' and 'double consonant' instead of 'geminate'. 'Long' is not the same as 'double', nor is 'single' the same as 'short'. Nobody ever calls a long vowel a double one, or a short vowel a single one. A long nasal is one phoneme and is in opposition to a short nasal; a double nasal is a group of the same phoneme twice. Long *n* (*n̄*) is an element of a phonological system, double *n* (*nn*) belongs to what is sometimes called phonological syntax.

However, are we certain that the author means a long nasal or liquid? With I, as appears from "or double consonant", he is thinking of cases like Gothic *þriskan*, Old Norse *bresta*, *gnesta*. But here we have not the same phoneme twice, but two different phonemes, therefore undoubtedly two phonemes. In that case we have to take it that by 'double nasal' he also means two phonemes. That such groups of phonemes occurred in Primitive Germanic should not be implied but proved or at least made acceptable. If we suppose that by 'double nasal' a long nasal is indicated, then the word 'double' does not retain its identity in I, and a higher degree of consistency is suggested by means of the ambiguity of a term than there really is.

3. The Primitive Germanic ancestors of such Old Norse verbs as *bresta* and *gnesta* are inadequately described by 'e + double consonant', for they also have in common that the vowel is preceded by a nasal or liquid. Seen diachronically, they would not, without this characteristic, have had the same categories as other verbs of the same group. Seen synchronically, this common characteristic is not less important than the one given in I.

4. In I, nasal and liquid are opposed to the consonants, in II ("liquid / nasal or *other* consonant"), they are included among them. This inconsistency can easily be avoided if we replace 'consonant' by 'spirant and (or) plosive'. The last two had better not be taken together as 'mutes', for this term shares in the turbidity of that of 'sonant' (see below), with which it is contrasted.

5. Whether by 'double nasal' we mean one or two phonemes, in either case we presuppose that a number of assimilations had taken place before the Primitive Germanic stage of which we are trying to describe the verbal system. If we characterize this stage by means of 'e and a' or of a four vowel system, we have dissociated ourselves

from the system of the consonants. In some cases we succeed in placing vocalic and consonantal sound laws in time with regard to each other. Thus, we know that the change $o > a$ must be older than the loss of a nasal before a breathed guttural spirant, and that the same applies to the change $\bar{a} > \bar{o}$. We can deduce this from a form like *brahta*, among others. If we look at it from the opposite point of view, we should expect a form *brohta**. Such an argumentation is impossible in the case of $o > a$ and, for example, $nw > \bar{n}$. I, therefore, contains a suppressed supposition which in itself is not unreasonable, but for which no clear argument can be adduced.

Of these five objections, the first is the most important. This will become more evident after an analysis of the way in which Van Coetsem further classifies the so-called 'e-group'. This classification is to be found on p. 9 of his study. It looks like this.

TABLE 6

$\frac{\text{I}}{1, 2, 3}$	$\left[e + \begin{cases} \text{sonant} + \text{consonant (sonant)} \\ \text{consonant} + \text{consonant} \end{cases} \right]$
$\frac{\text{II}}{4, 5}$	$\left[e + \begin{cases} \text{sonant} \\ \text{consonant} \end{cases} \right]$

It is clear that the *i*, *u*, liquid and nasal from table 5 have been taken together under one heading, 'sonant'. Now, this is one of the, not so very few, terms used in linguistics that do not come up to reasonable standards of scientific exactitude. On account of its, naturally relative, sonority, *n* is called a sonant. But we also speak of sonant *n*, and that only makes sense if there is also a non-sonant *n*. When we look at the publications on the subject we see that the situation is even more complicated. We find the term 'sonant' standing for: *n* in general; *n* in a language that has no variants of it; *n* in a language that has two variants of it; either of these variants. It goes without saying that the term cannot retain its identity in all these cases.

What meaning or meanings has Van Coetsem given to it? On p. 75 he says, "Für das Urgermanische nehme ich übrigens die phonologische Identität von *i*, *j* (j) einerseits und von *u*, *w* (w) andererseits an", and in other places, too, he gives evidence of this opinion. So in both cases there is one phoneme with two variants, one syllabic and one

non-syllabic. Apparently, it is regarded as a sonant because of its relative sonority as compared with other phonemes, therefore also when it appears in the present-root after *e*, in its least sonorous or sonantic form. For the same reason, the term is applied to the liquids and nasals. But we are not informed about their variants. Now suppose that in that Primitive Germanic stage these were no syllabic, no 'sonant' nasals and liquids. Then, $e + \underset{\cdot}{i}$ is only apparently the same as $e + n$, for $\underset{\cdot}{i}$ alternates with *i*, but *n* does not alternate with η . This is a relevant phonological difference, and one that should not be effaced by the use of the term 'sonant'. If in that period the (supposed) sonant *n* or (supposed) *en* had already become *un*, the series of categories of the verbs of the third class would show a completely different picture from that of the verbs of the first and second classes: the latter would have, in the third and fourth categories, one phoneme less than in the first and second; this does not hold good for class III. Compare: $e + \underset{\cdot}{i} + \text{consonant}$ — $a + \underset{\cdot}{i} + \text{consonant}$ — $i + \text{consonant}$ — $i + \text{consonant}$, with: $e + n + \text{consonant}$ — $a + n + \text{consonant}$ — $u + n + \text{consonant}$ — $u + n + \text{consonant}$. Such a difference in conjugation makes us abandon the attempt at reducing the root structures of these verbs to the same denominator by means of the term 'sonant' or any other term.

Those who think this argument correct cannot but come to the conclusion that the grouping together in I of table 6 is based on the notion: the *e* — *a* stage was also characterized by the presence of syllabic nasals and liquids. However, this notion has not been defended and is, I think, hardly defensible. If it were, this grouping together would be quite acceptable. But the conjugations of the fourth and fifth classes are different, of that much we are certain. Therefore they cannot be considered for a grouping together under II.

The classification of the strong ablaut verbs without reduplication, like that of the other verbs, should not be based on the form of the root but on the conjugation. The reconstruction of series of categories, as has been shown, is impossible if we do not first form an opinion on (part of) the Primitive Germanic vowel and consonant systems. And this in its turn compels us to form an idea of the chronology of certain sound laws. Van Coetsem's description of the vowel system seems to me to be correct. As to the consonant system: I do not believe that in the *e* — *a* stage there were syllabic (sonant) variants of the nasals and

liquids, nor do I believe that then *e* or *a* alternated with *ə* in the conjugations of the verbs of the third and fourth classes. So I start from the supposition that the change *ən* > *un* or (less probably) *ŋ* > *un* took place before *e* had become *i* in certain positions. Now what are, synchronically, the series of categories?

The consistent application of a synchronic method entails a modification or even complete rejection of the terminology usual in historical grammars. This has already turned out to be necessary with regard to 'preterite-present'. The term 'ablaut', too, carries a diachronic burden. For by it is meant: 'a vowel alternation dating from the Indo-European period'. Synchronically, this descent is irrelevant. I should like 'ablaut' to be understood as 'a functional alternation of vowels as it occurs in the language to be described'. In this connection the question may be put as to what we can do with the notion 'quantitative ablaut', and with such terms as 'lengthened', 'reduced' and 'zero' grades. Let us take Gothic *niman* — *nam* — *nemum* — *numans*. There is no reason why we should consider this as anything but an alternation of four different vowel phonemes, of four elements that have the same value and cannot be reduced to each other. This means that this type of conjugation is described adequately by 'i—a-ē-u', and that we can leave the consonant system out of consideration here, since the rest of the word remains unchanged. The same can be said of its Primitive Germanic equivalent *e* — *a* — *ē* — *u*. Undoubtedly, *e* is short and *ē* is long. But this does not entitle us to speak of a normal and a lengthened grade. The only thing that is relevant is the fact that they are different. In this respect their mutual relation is not different from their relation to *a* and *u*. In such cases, I should prefer not to speak of 'quantitative ablaut', which term I should like to reserve for the alternation of two phonemes with one phoneme. In the case of the Primitive Germanic verbs of the first and second classes, it is not sufficient to give *e* — *a* — *i* — *i* and *e* — *a* — *u* — *u*, we must also give the phoneme following *e* and *a* (see above). We can leave the rest of the root out of consideration.

So far, my observations on the ablaut verbs without reduplication have, within the limits set by Van Coetsem's publication, almost exclusively been restricted to the first five classes. But of course the sixth class should also be dealt with. Moreover, we should include

the residual groups in our systematic description (see above, p. 242) ²⁾. I am referring to the so-called 'aorist-presents', which are described in the handbooks as 'strong verbs with reduced or zero grade in the present'. The term is even more unfortunate than 'preterite-present'. It can only be used in the case of a verb that has, by the side of other forms, an aorist which is used for an action taking place in the present. But it has never been proved that there were verbs in Primitive Germanic which had an aorist in addition to a present and a preterite. The present of what is designated by the unfortunate term cannot, therefore, be compared with the aorist of another verb in the same language. On the other hand, the present of a weak verb with internal ablaut, provided we abstract from the function, can in a number of cases be compared with the preterite of another verb in the same language, because that preterite at any rate exists. For this reason, many scholars will probably prefer to preserve the term 'preterite-present' in spite of my above objections. The other term they will probably reject just as I do. For a synchronic description of a period in a Germanic language it cannot be used. At best, "this Gothic or Primitive Germanic verb is an aorist-present" can mean "it used to be one when it was a different word in a different language".

For that matter, we do not really need a comprehensive term. It has not yet been proved that they form a group, contrasting with other groups that are formed on the basis of the same criterion. For the present they remain separated from each other because of a difference in their series of categories. Not everything is quite certain here. We are pretty well convinced of the existence of Primitive Germanic verbs with *ū* in the present. A verb like Old Norse *koma*, too, will have had a non-defective ancestor. It is doubtful, however, if Gothic *ḍigan* may be traced back to a Primitive Germanic verb with four categories. If we assume that it can and if we accept the ancestor of the verb which in Gothic is *itan* as a partner of equal value, we have to take it that in the *e* — a period there were the following ten series of categories:

²⁾ This is also the opinion of C. B. van Haeringen, as appears from the classification of the Dutch strong verbs devised by him (*De Nieuwe Taalgids*, XLIII, p. 20 ff.).

TABLE 7

	1	2	3	4
I	e + i	a + i	i	i
II	e + u	a + u	u	u
III	e	a	u	u
IV	e	a	ē	u
V	e	a	ē	e
VI	a	ō	ō	a
VII	e	ē	ē	e
VIII	i	a + i	i	i
IX	ū	a + u	u	u
X	u	a	ē	u

How can this be reduced to a system? It can be done in more than one way. As a primary criterion could be taken: equality of and difference between the members of each series. If we indicate these members by different letters when they are different, and with the same letters when they are equal, the ten series may be represented as follows:

TABLE 8

I	p	q	r	r
II	p	q	r	r
III	p	q	r	r
IV	p	q	r	s
V	p	q	r	p
VI	p	q	q	p
VII	p	q	q	p
VIII	p	q	p	p
IX	p	q	r	r
X	p	q	r	p

It would seem possible to reduce them to five groups: p-q-r-r (I, II, III, IX), p-q-r-s (IV), p-q-r-p (V, X), p-q-q-p (VI, VII), p-q-p-p (VIII), with three, four, three, two and two different members respectively. Systematically:

TABLE 9

4	A	p	q	r	s	(IV)
3a	B	p	q	r	r	(I, II, III, IX)
3b		p	q	r	p	(V, X)
2a	C	p	q	q	p	(VI, VII)
2b		p	q	p	p	(VIII)

I am fairly certain that this is an acceptable principle of arrangement. However, the question remains as to whether it can be considered as primary. No account has been taken of the fact that the members of a series of categories show functional differences and that the opposition between two phonemes and one phoneme is not the same as the opposition between one phoneme and another. If we use the notion of internal ablaut (this time: in the preterite), and if we also accept quantitative ablaut as a, be it secondary, criterion, the system will look completely different. In that system we can also make use of the data of table 9. Once more it will appear that a certain characteristic can be used for further classification in some cases and not in others. This is in accordance with what we saw in tables 2 and 3. Only, the variation on the theme 'two characteristics – three groups' is more intricate here than in the other cases. The system – or rather, the systematic description – has now become:

TABLE 10

Strong ablaut verbs without reduplication

with internal ablaut						without internal ablaut	
with quantitative ablaut		without quantitative ablaut				without quantitative ablaut	
2b	3a	3		4		2a	
		a	b				
VIII	I II	III IX	V X	IV		VI	VII

The relations between the members of the series of categories (table 8) have consistently been made the basis of this classification; it is only in the last resort that these series appear in their concreteness. Where possible, agreement has been reached with the classification of the weak verbs, while for the further classification of the verbs with internal ablaut (table 2), the notion of the (synchronic) quantitative ablaut can also be used, so that the Primitive Germanic ancestors of Gothic *wait* – *witum* are opposed to those of Gothic *skal* – *skulum*. Furthermore, all series have been treated as being essentially equivalent, and no account has been taken of the fact that some comprise many verbs, others only few.

According to the usual views there are six classes; VII, VIII, IX

and X are relegated to different series. The six classes can be divided into two groups: the first five as opposed to the sixth, the e — a group as opposed to the a — ō group (cf. above, p. 245). If we start from the same suppositions as in the case of tables 7–10, we get the following result:

TABLE 11

A	I	e	i	a	i	i	i
		i		a	i	i	i
	II	e	u	a	u	u	u
		ū		a	u	u	u
	III	e		a		u	u
	IV	e		a		ē	u
		u		a		ē	u
	V	e		a		ē	e
		e		ē		ē	e
	B VI	a		ō		ō	a

In this way, the e — a group also contains verbs that have either no e in the present or no a in the preterite singular. This is illogical. Undoubtedly, their series have three members in common with one of the other series. This is indeed something, but it is not enough. Those who wish to work consistently with this criterion — and what is a system without consistency? — will have to regard IV as a variant of V, or V as a variant of IV, and will then immediately get into difficulties with the series they had already accepted as variants. In short: if we want a truly synchronic description, and if we wish to include in that description the residual series as equal partners, we shall meet with unsurmountable obstacles when we take the vowel of the present or the vowels of the present and preterite singular as primary criteria. Moreover, it is hard to see why a certain vowel of the present should be more relevant than phenomena such as internal ablaut, quantitative ablaut and number or order of different members of the various series.

Needless to repeat, my attempts at a systematization of the strong ablaut verbs without reduplication are based on Van Coetsem's suppositions: Primitive Germanic had a four vowel system, and in the first two series two phonemes alternated with one phoneme. Another

supposition is that in that period sonant *n* etc. or *ən* etc. had already become *un* etc.

Although I do not agree with Van Coetsem's classification on the basis of the structure of the root syllables, without his publication this article would not have been written.

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BALTIC *EI* AND DEPALATALIZATION

Summary

The author posits that in word final position the Common Baltic group ***-*Cjai* > **-*Āei* > *-*Cei*, i.e. in the final step palatalization was lost. Etymological depalatalization is found in those cases where East Baltic *-*Āei* passed to *-*Cie*, cf. nom. pl. masc. *pēsti* < **pēstie* < ***pēstjai* and in the dat. sg. of -*ē* stem nouns, e.g. *vērtēi* < **vertjai*. In some cases the palatalized consonants have been restored by analogy, e.g. *valdžiai* (not **valdei* as our theory supposes) with *dž* on the basis of the nom. sg. *valdžià*. The *-*ē* (Lith. -*ė*) stem nouns owe their origin to analogy with the *-*ā* stems, i.e. -*Cai* is to *-*Cā* as -*Cei* is to *-*Cē*.

The fate of the Baltic diphthongs *-*ai*- and *-*ei*- has been discussed frequently and the opinions on the matter are various. Endzelin supposes that East Baltic -*ie*- developed from an *-*ē*- which in turn came from an *-*ēi*- in stressed position ¹). Pranas Skardžius has recently confirmed this view in an article in which he says that in Finnic borrowings from Baltic an -*ai*- (e.g. Finn. *taivas* 'heaven') does not necessarily presuppose a Baltic *-*ai*-, but may go back to Baltic *-*ei*- ²). In this manner Skardžius shows that Lith. *diēvas* 'God', Lett. *dīevs* 'id.' does not necessarily reflect Baltic **daivas*, but may reflect **deivas* instead. On the basis of certain Old Lithuanian texts Stang, however, has established a paradigm for the verb *mėgsti* 'to like' with a 2d sg. pres. **mėgstai* ³). Thus Stang says, "Dies deutet darauf, dass die Endung -*i*, < -*ie* (vgl. refl. -*iesi*) der 2. Person unmittelbar auf -*ai*, nicht auf -*ēi* zurückgeht" (*ibid.*, p. 138). In Stang's opinion, then, Lithuanian -*ie*- may result from an earlier *-*ai*-, although the exact conditions for this change are not established. A more extreme opinion has been advanced by André Vaillant, who says that every original **ai* and **ei* became *ie* in East Baltic ⁴). Where we find modern Lith.

¹) J. Endzelins, *Latviešu valodas gramatika* (Riga, 1951), pp. 59-60.

²) "Zum Wandel *ai*, *ei* > *ie* im Litauischen", *Zeitschrift für slavische Philologie* 26 : 275-82.

³) "Ein Beitrag zum Problem vom Diphthong *ie* im Litauischen", *Die Welt der Slaven* 1 : 137-139.

⁴) *Grammaire comparée des langues slaves* (Paris, 1950), p. 115, 118.

and Lett. *ai* and *ei*, it was restored on some kind of analogical basis. We are inclined to accept Stang's opinion that East Baltic *-ie-* may arise either from **-ai-* or **-ei-* under circumstances which are not yet clear, although Vaillant's position that every original **ai* and **ei* became *ie* has points in its favor.

It is generally accepted that Prim. Baltic **-j-* was lost in position following a consonant and before a front vowel. To put it differently, the groups **Cje* and **Ce* merged. Once this had happened the *-a-* in the group **Cja* had greater allophonic freedom, since after the group **Cj-* there was no longer any chance of contrast between **/e/* and **/a/*. To schematize the situation we present the following diagram:

Period I

(Contrast between **/a/* and **/e/* in all positions)

<i>*Ce</i>	<i>*Cje</i>
<i>*Ca</i>	<i>*Cja</i>

Period II

(Contrast between **/a/* and **/e/* after *C*; no contrast after *Cj-*)

<i>*Ce</i> (< <i>*Ce</i> and <i>*Cje</i>)	
<i>*Ca</i>	<i>*Cja</i>

At this point there exists the possibility of three contrasting groups with the diphthongs *-ai-* and *-ei-*, viz. **Cai*, **Cjai* (or better *Ĉai*) and **Cei*. We propose that at least in final position there was a further merger in Prim. Baltic, i.e. that the group **Cei* merged with **Ĉei* (< **Cai* < ***Cjai*) to produce **Cei* (with unpalatalized *C*)⁵.

The major argument against this proposal is that we find the group *Ĉei* in Baltic, e.g. in Lithuanian we find the nom. pl. masc. **-jo-* stem noun *žōdžiai* 'words' (pronounced *žōdžēi*, seemingly to be derived from

⁵ Prim. Baltic **Ce* (< P.I.E. **Ce* and **Cje*) and **Cja* have merged in modern standard Lithuanian as *Ĉe* (except in the case of **/dja/* which has passed to */dže/* and **/tja/* which has passed to */če/*, whereas **/de/* has passed to */de/* and **/te/* to */te/*). In modern Žemaitish dialects the merger has been complete (including **/tja/*, **/te/* and **/dja/*, **/de/* respectively), but the result has been unpalatalized *Ce*. Although the aforementioned phonemic mergers have nothing to do directly with our proposal, they are interesting parallels showing the tendency of the Baltic languages to lose the distinction between palatalized and unpalatalized consonants before front vowels.

žodjai*), and the dat. sg. fem. *-*jā* stem *valdžiai* 'to the government' (pronounced *valdžei*, seemingly derived from **valdjai*). It is to be noted that the examples are from flectional forms, where, in our opinion, analogy restored the palatalized consonant ⁶). In standard Lithuanian, the nom. dual of *valdžià* is *valdì* (< **valdei* < *valdjai* without the etymological palatalization). Wiedemann lists the dual *valdži* ⁷) and comments that one would expect **valdì*, although the asterisk is misleading because the standard form is *valdì* ⁸). Wiedemann reasons, however, "in *valdžì* ist *dž* (i.e. our *dž*) aus den übrigen kasus übertragen" ⁹). We believe that his comment can be applied to the palatalization found in the dative case of the same word, i.e. the *dž* of *valdžiai*. Analogical restoration of stem final consonants is, of course extremely common. One might compare the case of modern Russian noun paradigms with stem final -*k*, -*g* and -*x*, where almost all trace of the second palatalization has been lost, e.g. Russ. masc. nom. pl. *bogi* 'gods', but O.C.S. *bodzi* (< **bogoi*) or Russ. fem. dat. sg. *ruke* 'to the arm', but O.C.S. *rqčě* (< **ronkai*). Although the existence of the Russian phonemic sequence /gi/ can be explained as the result of certain

⁶) Phonemic sequences which are not expected from the standpoint of the historical phonological development of a language may be created in several ways. Borrowing is one route. In modern standard Lithuanian we find the word *čigėnas* 'gypsy' in which *č*- stands before -*i*-, a combination which would be impossible from the historical point of view. Another route is the creation of new morphemic sequences. Etymologically standard Lith. *dž* arose from **dj* in position before a non-front vowel. Nevertheless we find in the standard language a compound *mėdžliepis* 'maple tree' in which *dž* stands before *l*. It is true that there is a plus juncture between the *dž* and the *l*, but the important point is that *dž* should not even occur before a plus juncture from the historical point of view. It is possible to imagine (I am not predicting, just suggesting the theoretical possibility) that at some future date the plus juncture between *medž* and *liepis* would be lost.

⁷) *Handbuch der litauischen Sprache* (Strassburg, 1897), p. 53.

⁸) cf. A. Senn, *Kleine litauische Sprachlehre* (Heidelberg, 1929), p. 25.

⁹) Friedrich Kurschat, *Grammatik der litauischen Sprache* (Halle, 1876), p. 172, says, "Den Lautgesetzen zufolge sollten der N.A.V. Dual von *valdžià* nicht *waldžì* sondern *waldì* lauten . . . aber es findet hier eine Ausnahme statt, vielleicht in Folge dess, dass der Dual nur selten vorkommt und daher das betreffende Lautgesetz nicht genügend Gelegenheit gehabt, sich hier geltend zu machen". Although it is interesting that Kurschat lists the form, it is clear that nowadays most linguists would doubt his reasoning here.

regular sound changes (i.e. /gi/ < /gy/ < */gū/), the same cannot be said for the group /ke/.

Loss of the phonemic distinction between the consonants in the phonemic sequences *-Āei (< **Cjai) and *-Cei does not seem improbable in the light of the history of the Baltic languages. The simplest explanation for the nominative and dative plural masculine of the *-jo stem adjectives in Lithuanian is to assume East Baltic depalatalization before the ending -i (< *-ie < **-jai). For example, Lith. *pėsčias* 'on foot' (< *pēst-j-as) has the nom. pl. masc. *pēstì* (< *pēstie < **pēst-j-ai), dat. pl. masc. *pēstiams* (< *pēst-j-aimus), cf. the O.C.S. nom. pl. masc. *vyšīnii*, dat. pl. *vyšīnimū* (with palatalization of the stem final consonant). A similar depalatalization is apparently observed in the 2d sg. of the present conjugation of the *-je/o verbs, e.g. the present conjugation of *āusti* 'to weave' is (1st. sg.) *āudžiu* (< *aud-j-ō), 2d sg. *āudi* (< *aud-j-ie < **aud-j-ai), etc. In this case there was a complete phonemic merger of the diphthongs *-ai- and *-ei-, cf. the first paragraph of this paper, but it should be noted that there was also loss of palatalization of the preceding consonant.

There are numerous theories regarding the origin of the Baltic *-ē stem nouns. Ferdinand Sommer suggested that under certain conditions word final *iā passed to *-ē, whereas under other conditions this shift did not take place¹⁰). According to Sommer, the development of *iā depended upon syllable division in Prim. Baltic. iā did not pass to ē if it began a syllable, i.e. either if there was a preceding vowel, e.g. Lith. *žvejà* 'fishing'; or if there was a preceding diphthong, e.g. *saujà* 'hollow of the hand'; or if there was a preceding so-called 'Liquida-diphthong' (i.e. one ending in -r, -l, -n), e.g. *gir-ia* 'forest', *žin-ia* 'information'. If the consonant preceding word final *iā began a syllable, then an *ē developed, e.g. the nom. sg. fem. *di-diā* (syllable division marked by a hyphen) passed to *di-dē*. Although in general *iā did not pass to *ē if a consonant closed the syllable (the gen. sg. fem. of *didē* is *didžiōs* < *di-diās), it did in the acc. pl. fem. as a result of the etymological 'Stosston' accent, i.e. *didēs* < *didiās. From the preceding theoretical considerations, Sommer establishes a paradigm with the following historical stages.

¹⁰) *Die indogermanischen iā- und io- Stämme im Baltischen* (Leipzig, 1914), pp. 78-87 and p. 368.

Singular	Nom.	Gen. Plural	Nom.	Acc.
(1)	*di-diā	*di-diās	*didiās	*di-diās
(2)	*didē	*didiās	*didiās	*didēs
(3)	*didē	*didiās	*didiās	*didēs
(4)	didē	didžiōs	didžios	didēs

The forms listed above after (4) correspond roughly to those given by Kurschat in his grammar, according to Sommer (*op. cit.*, p. 85).

Pranas Skardžius contests the distribution of case endings as listed by Sommer and gives numerous Old Lithuanian examples of the vacillation between the *-ē* and *-iā* stems ¹¹). For example, in Daukša's *Postille* one finds the gen. sg. fem. *didēs*, nom. pl. *dides* (opposed to the gen. sg. *didžiōs*, nom. pl. *didžios*, given by Sommer); in Širvydas we find as fem. gen. sg. both *didēs* and *didžios* (Skardžius, *op. cit.*, p. 172). According to Skardžius, the co-existence of *-ē* and *-iā* stem forms in the same paradigm is the result of a later mixture, rather than the split of a single original paradigm (*ibid.*). Skardžius traces the Lith. *-ē* stems directly back to P.I.E. **ē* and **iē* stems (p. 175), cf. Gk. *xrē* 'it is necessary' (< **ghrē*), *pénē-s* 'poor' (< *penē-*), *peinē-n* 'to be hungry' (< **peniē*).

Endzelin disputes Sommer's statement that there are no Latin words in *-ēs* which would completely correspond to Baltic ¹²). According to Endzelin (*op. cit.*, p. 292-3), this isn't quite true, since Old Lat. *facēs* 'torch' with the exception of the final *-s* corresponds exactly to Lith. *žvākė* 'candle'. Endzelin adds that, of course, the number of Latin words corresponding to Baltic *-ē* stems is small, but so is the number of Latin *-u* stems corresponding to Baltic *-u* stems very small. The only example is Lat. *pecu*, O. Pr. *pecku*, Lith. *pekus* 'cattle', but even this example is dubious because of the Baltic *-k-* (for expected Lith. **-g-* < P.I.E. **-k-*). Endzelin does not mention one counter-argument which seems of importance. The *-u* stems are a well established Indo-European flectional category, whereas the evidence for the **-ē* stems is not nearly as clear. As Sommer says, "Ich glaube nicht, dass jemand den Mut besitzen würde, uns ohne die Hilfe des Baltischen und Latei-

¹¹) "Zur Entstehung des *ē*-Ausganges im Litauischen", *Zeitschrift für slavische Philologie* 23: 171-176.

¹²) "Zametki o baltiiskix imennyx osnovax na *-(j)ē* i *-jo*", *Russkij filologičeskij vestnik*, Vol. 76: 292-315.

nischen, etwa vom Germanischen und Keltischen aus, mit einer idg. $\dot{i}\bar{e}$ -Klasse zu beschenken". (*op. cit.*, p. 9).

It seems clear that there is some kind of relationship between the $*\bar{e}$ and $*\dot{j}\bar{a}$ stems, because of the frequent vacillation between the two, e.g. in standard Lithuanian we find *mùsė* 'fly', *kaltė* 'fault', *vertė* 'value', but in dialects *musià*, *kalčià*, *verčià* ¹³). In a case where two paradigms are related, either later mixture or an early split is theoretically possible and no absolute proof can be given for either assumption. We are inclined to agree with Sommer in as far as he believes that there is a common origin for $*\dot{j}\bar{a}$ and $*\bar{e}$ stems. In the first place, as we mentioned in the previous paragraph, the relationship between Baltic $*\bar{e}$ nouns and $*(\dot{i})\bar{e}$ nouns in other Indo-European languages is rather tenuous. Likewise we are impressed by the numerous morphological parallels between the $*\dot{j}\bar{a}$ and $*\bar{e}$ stems, e.g. *ōrė* 'plowing time', *kūlė* 'threshing time', but *sėjà* 'sowing time' (cf. Sommer, *op. cit.*, p. 11). Indicative also is the relationship between adjectives with an etymological $*\dot{j}o$ stem in the masculine, but with a corresponding $*\bar{e}$ stem in the feminine, cf. masc. nom. sg. *medinis* 'wooden', gen. *medinio*, but fem. nom. sg. *medinė*, gen. *medinės*.

On the other hand we contest Sommer's belief that the creation of the $*\bar{e}$ ($< *i\bar{a}$) in Baltic depends upon the syllabic grouping. Particularly do we question the positing of three types of phonetic treatment of $*/\dot{i}/$ ¹⁴), viz. $*gir'ā$, $*žemiā$ ($> žemė$) and $*saulia$ ($> saulė$). According to Sommer, the fact that $-j-$ is still pronounced after labials and before back vowels in Lithuanian allows one to posit a special original kind of pronunciation for $*žemiā$ (*op. cit.*, p. 79). This is why an original $*žemiā$ passed to *žemė*, whereas $*giriā$ passed to *girià* = $/\dot{g}i\bar{r}\bar{e}/$. In the case of $*žemiā$ the labial $-m-$ caused the passage to $-\bar{e}-$, but in the case of $*giriā$ the $-\dot{i}-$ was lost with concomitant palatalization of the $-r-$. This seems to be one of the weakest parts of Sommer's thesis and has apparently convinced few people in the past ¹⁵). Similarly it seems

¹³) Jan Otrębski, *Gramatyka języka litewskiego*, Vol. III, (Warsaw, 1956), p. 36.

¹⁴) We do not believe that there was a P.I.E. phoneme $*/\dot{i}/$ distinct from $*/j/$, so we write $-j-$ where Sommer uses $-\dot{j}-$.

¹⁵) N. van Wijk, "Lit. *žemė*, aksl. *zemlja* und das Problem von der baltischen $\bar{e}$ -Deklination", *Archiv für slavische Philologie*, 42: 286-9. Endzelin (*Zametki*, p. 298) feels that Sommer does not satisfactorily explain away the counter

unlikely that the nom. sg. fem. **di-diā* would have a different development from that of the gen. sg. **di-diās*.

Returning now to our own hypothesis, we believe that after the passage of **Cjai* to **Cei*, the nouns with stems in **-jā* in post consonantal position were declined in the following manner:

Singular	Plural	Dual
N. <i>*-Cā</i> (< <i>**Cjā</i>)	<i>*-Cās</i> (< <i>**Cjās</i>)	<i>*-Cei</i> (< <i>**Cjai</i>)
G. <i>*-Cās</i> (< <i>**Cjās</i>)	<i>*-Cōn</i> (< <i>**Cjōn</i>)	
D. <i>*-Cei</i> (< <i>**Cjāi</i>)	<i>*-Cāmus</i> (< <i>**Cjāmus</i>)	<i>*-Cām</i> (< <i>**Cjāma</i>)
A. <i>*-Cān</i> (< <i>**Cjān</i>)	<i>*-Cans</i> (< <i>**Cjans</i>)	<i>*-Cei</i> (< <i>**Cjai</i>)
I. <i>*-Cān</i> (< <i>**Cjān</i>)	<i>*-Cāmis</i> (< <i>**Cjāmis</i>)	<i>*-Cām</i> (< <i>**Cjāma</i>)
L. <i>*-Cei</i> (< <i>**Cjai</i>)	<i>*-Cāsu</i> (< <i>**Cjāsu</i>)	

For the **-ē* stems we can reconstruct an original **-ei* in the loc. sg. ¹⁶). For the **-jā* stems a dual nom. acc. fem. **-Cjai* can be reconstructed on the basis of Slavic *zemlī* 'two lands'.

In the **-ā* stem declension the nom. sg. is **-Cā*, gen. sg. **-Cās*, dat. sg. **-Cai*, loc. sg. **-Cai* ¹⁷), nom. acc. dual **-Cai* (cf. O.C.S. *ꙗꙗcě*). We suppose that in the **-Cā* stems those forms with **-Cei* led to the creation of a new nom. sg. **-Cē*, gen. sg. **-Cēs* on analogy with the **-ā* stems, i.e. **-Cai* is to **-Cā*, **-Cās* as **-Cei* is to **-Cē*, **-Cēs*. Sometimes the analogical change did not take place and eventually a palatalized consonant was substituted before the endings with **-ei*, e.g. dat. sg. *valdžiai* from *valdžiā* 'government'. This explains why we have the frequent alternation between **-jā* and **-ē* stems. Presumably the analogical **-ē* stems and the original etymological **-jā* stems were in competition. By contrast with the old etymological **-jā* stems, the new **-ē* became a productive suffix deriving feminines from masculines, cf. Lith. masc. nom. sg. *vilkas* 'wolf', but *vilkė* 'she-wolf' and Lith. masc. nom. sg. *ėlnias* 'Cervus elaphus' or *ėlnė* 'female of the same species'.

According to André Vaillant, "Après une consonne mouillée, un *a*

examples where **-njā-*, **-ljā-*, **-rjā-* do not pass to **-n'a-*, etc., e.g. Lith. *karė* 'war', *pelė* 'mouse', *gilė* 'acorn'.

¹⁶) J. Endzelins, *Baltų kalbų garsai ir formos* (Vilnius, 1957), p. 117 = the Lithuanian translation of his *Baltų valodu skanas un formas*.

¹⁷) Endzelins, *op. cit.*, p. 114.

balto-slave devenait 'a et se confondait avec e ...' 18). We doubt therefore that there was a contrast between **-jai* and **-jei* in Common Baltic. The **-ē* stems only developed where there was a clear phonemic contrast between /ai/ and /ei/, i.e. after an unpalatalized consonant. In those cases where a vowel immediately preceded the *-j-*, e.g. **kājā* 'leg', a form such as the dat. sg. **kājei* (cf. modern Lith. *kójai* = /kojei/) merely contained the only possible phonetic realization of the morpheme **-ai* in position after *-j-*. To put it another way, the stem final *-j-* determined the existence of the **-ei* automatically, since **-ai* was not possible after *-j-*. This differs from the situation of the **-ē* stems (etymological **-Cjā* stems) where the **-ei* (< **-Cjai*) was not determined automatically once the palatalization of the preceding consonant was lost. This is the reason that the *-ē* stems never developed for words with stem final *-j-* preceded by a vowel.

For our purposes it is useless to review all of the theories of the origin of the Baltic preterit in **-ē* 19). It is interesting to note that Kurschat already had proposed that the preterit endings (1st sg.) *-C-au*, (2d sg.) *-C-ei*, (3d sg.) *-C-ē* take their origin in the forms **-iai*, **io*, i.e. the palatalized counterparts of the preterit of verbs with unpalatalized stems 20). According to Kurschat, "Das *-iai* ist hier demnach so vollständig in *-ē* übergegangen und das Bewusstsein der ursprünglichen *ia*-Erweichung ist der Sprache so gänzlich entschwunden, dass vor diesem *ē* nicht einmal *d* und *t* in *dž* und *cž* (*č*) übergehen, was doch in der Declination z.B. bei *žodžiei* geschieht". (*op. cit.*, p. 280). We do not propose to follow Kurschat inasmuch as he seems to suggest sporadic sound change. Our proposal, however, does suggest a relationship between the etymological *-je/o-* stems and the preterit in **-ē* (Lith. *-ė*). Parallel to the etymological **-ā* preterit with the 1st sg. **-C-au*, 2d sg. **-C-ai*, 3d sg. **-C-ā* there was a **-jā* preterit with the 1st sg. **-Cj-au*, 2d sg. **-Cj-ai*, 3d sg. **-Cj-ā*. First the 2d sg. **-Cjai* passed to **-Cei*, and next on analogy with the **-ā* preterit (2d sg. *-ai*, 3d sg. *-ā*) the 3d sg. pret. **-Cē* was created. The **-ē* preterit was not created for verbal roots with post-vocalic *-j-*; the reason is the same as for nouns in *-j-*, i.e.

18) *Grammaire comparée*, p. 109.

19) A good account of this can be found in Edmund Sandbach, *Die indogermanischen zweisilbigen schweren Basen und das baltische (litauische) Präteritum* (Heidelberg, 1930), pp. 3-11.

20) *op. cit.*, p. 280.

**ei* is not phonemically different from *-*ai* following -*j*- where the contrast between */*ei*/ and */*ai*/ was neutralized.

It seems clear that in a few cases the Baltic *-*ē* preterit derives directly from a Prim. Indo-European *-*ē*-, cf. Gr. aor. *ēmánēn* for *maínomai* 'I rage' ²¹). In the great majority of cases, however, a purely Baltic origin is to be posited for the *-*ē* preterit, particularly in view of the fact that the phonological conditions for its distribution are the same as for the noun in *-*ē*, i.e. *-*ē* does not occur after post-vocalic -*j*-. Likewise the Greek aorist in *-*ē* is passive whereas the Baltic preterit in *-*ē* is primarily transitive ²²). Stang notes that the *-*ē* preterit in Baltic belongs mainly to the verbs with a present in -*je/o*- which are mainly transitive. In our opinion, the *-*ē* preterit, which had its origin with *-*je/o*- verbs, then spread to other verbs (e.g. *dāvē* 'he gave' from *dúoti* 'to give', *dúoda* 'he gives', etc.) where it also serves as a marker of the transitive as opposed to the preterit in *-*ā* which in contrast has come to mark the intransitive.

In conclusion, then, we believe that there are two arguments in favor of the hypothesis that there was a phonemic merger of *-*Čei* (< *-*Cjai*) and *-*Cei*: (1) the tendency of the Baltic languages to abolish the opposition of palatalized vs. unpalatalized consonants before front vowels and (2) the fact that such an assumption explains the distribution of the *-*ē* stem nouns and the preterit in *-*ē*.

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²¹) Otrębski, *op. cit.*, p. 212.

²²) J. Endzelin, "Zum lettischen Präteritum", KZ 43: 34.

²³) *Das slavische und baltische Verbum* (Oslo, 1942), p. 189.

EXPRESSIVE DERIVATION OF PROPER NAMES IN RUSSIAN

The literature about derivations which, to use the terminology of Russian grammarians, express subjective evaluation is far from extensive. Of the older publications on the subject may be mentioned as the most important those by Belic (*Archiv für Slavische Philologie*, 1910), Mandelstam (*Žurnal Ministerstva Narodnogo Prosvěšćenija*, 1903) and Lettenbauer (*Das Deminutivum im Russischen*, 1933). To our knowledge only two authors have studied these forms from the viewpoint of structural linguistics, i.e. Karčevskij (*Charisteria Guilielmo Mathesio*, 1932) and Stankiewicz (*Word*, 1954).

Still scantier is the literature about similar derivations from proper names. In an article about Russian first names by Černyšov (*Revue des Etudes Slaves*, 1934) expressive derivations are mentioned only incidentally. Karčevskij in his above mentioned article devotes to them not more than a few lines, whereas Stankiewicz does not discuss them at all. In grammars proper names are, as a rule, only mentioned casually as a special kind of substantives, while their derivations are passed over in silence or at best disposed of with a few remarks. Yet, as we hope to point out in the following, Russian proper names are entitled to more attention than they have got so far, as their derivational forms show various deviations as compared to generic names.

Expressive derivations of proper names belong to the intimate, personal sphere. Accordingly, to study them one cannot rely on texts. The only way to gather the necessary data is by oral information. In our case we worked with three female informants. One of them had left Russia for good in 1943, while the other two, although living abroad, had been in their native country also after the war. With these informants we went through a list of a hundred odd names culled from the *Svjatcy*, the official list of names of the Greek Orthodox Church, and noted every derivation of them that they knew of. After we had crossed out those where the informants clearly showed hesitation, evidently not being familiar with them, there remained 75 names in all. The material we acquired is consequently of a restricted and more or less provisional character. The distribution of names, neutral and expressive ones alike, shows considerable variety. Certain

names are confined to certain social or local groups. Thus names like Eremej, Zachar, Karp, Kondrat, Feodosia are mainly heard in rural areas, among country people and are felt as common or vulgar by other, more sophisticated groups of the population. Others one hears exclusively in the cities or among intellectuals, others again one may come across in all walks of life. A number of names were – and perhaps still are – strictly confined to monks. A really exhaustive inquiry should, of course, have to take all these factors into consideration. We got the impression that the number of names now in use in Sovjet Russia is relatively small. It seems that people stick to the existing ones and that the introduction of new names is nowadays definitely not encouraged, if not actually forbidden, in contrast to the first years after the revolution.

Apart from the social and local divergence already mentioned there are other differences, especially in the use of expressive names, depending on sex, temperament, age and cultural background. Certain persons may show partiality to certain expressive names, which are seldom or never at all employed by others, as appeared clearly during the inquiry. In this respect too, accordingly, the method of inquiry imposed certain restrictions. It is, for instance, entirely conceivable that the results would have turned out differently, if we had worked with masculine instead of female informants.

Before entering into detail let us begin with a few preliminary remarks. Proper names have a full, “neutral” form, a short, familiar form and a large number of expressive forms derived from the latter which give expression to the attitude of the speaker towards the person addressed. A number of names have several of such short forms each with its own series of expressive derivations. Masculine and female names are not formally distinguished, neither in short nor in expressive names. The distinction may only come to light syntactically. Next to short names with their derivations we find sometimes expressive names directly derived from full names, but compared to the former the latter are not very frequently used. Expressive names fall into two clearly marked off kinds: hypocoristic and pejorative names. The latter may, under special circumstances, also serve as hypocoristic names. Between full, short and expressive names shiftings may occur, so that, for instance, a name formally belonging to short or expressive names become more or less neutral.

The choice of an expressive name may, of course, be affected by all kinds of different factors. As we all know from personal experience, nearly every name has its own – individually often widely divergent – evocative power, depending from our personal experience with bearers of the name, but also from rhythmic and acoustic effects. Certain names have a peculiar flavour, such as Tamara and Anna, which by my informants were considered as stately and beautiful, while others are felt as downright ugly. Furthermore the emotional value of a name may also be determined by accidental sound-correspondence with “ordinary” words. On the one hand a name may be avoided on account of a possible association with a certain word, on the other hand it may for that very reason be picked out. A good example is Charja, short form of Zachar (charja meaning “mug” (face)). Likewise the occurrence of a name in popular expressions and proverbs may occasionally affect its emotional value. Thus the names Kuz'ma and Makar had for my informants obtained an unpleasant connotation because of their association with pokazat' Kuz'kinu mat' and na bednogo Makara vse šiški valjatsja. That all such factors make themselves felt in the use of expressive names needs hardly be pointed out. Some further examples will be given later on in our discussion of the individual formations.

As expressive names are preferably derived from short names, we shall discuss the latter first. Short names consist almost invariably of two open syllables with final vowel a. As example may serve: Varvara: Varja; Vadim: Vadja; Ol'ga: Olja; Raisa: Raja; Fjodor: Fedja. From these examples we observe that the process of shortening consists in retaining the first syllable of the full name, plus the next consonant when that syllable is open, and adding to it the vowel a. The same device is applied in the case of monosyllabic names – which are rather rare – the difference being only that the “short” name is now actually longer than the full name: Lev: Ljova; Pjotr: Petja. Do we have to consider the final vowel as a suffix as some authors do? It seems more plausible to assume that in the process of shortening the tendency comes to light to reduce the full name to a well known expressive wordpattern with predilection for disyllabism, open syllables and identical consonants ¹). The last mentioned characteristic we meet in

¹) Compare Gonda, *The comparative method as applied to Indonesian languages*, Lingua I 1.

short names as Vova from Vladimir, Koka from Nikolaj, Lolja from Ol'ga, Lelja from Elena.

The fixed structure of the short name may result in one and the same name doing duty for two or more full names, e.g. Alja for Aleksandr (or Aleksandra) and Aljona, Koka for Nikolaj and Konstantin.

The accent of the short name is always on the penultimate irrespective of the accentuation of the full name. The consonant immediately preceding the final vowel may be hard or soft, apparently without any regularity.

Next to the short name a monosyllabic name may occur formed by casting off the final vowel: Griša:Griš, Vanja:Van', Nata:Nat. This name serves exclusively as call name and is not subject to declension.

As already will have become evident from some of the foregoing examples, the process of shortening is subject to various minor "irregularities". Of these we mention only the most important. Sometimes the second syllable is retained: Nikolaj:Kolja, or the final one: Vladislav:Slava. The initial consonant may be modified: Georgij:Žora, Evgenij:Ženja, or the vowel of the first syllable: Tamara:Toma.

A number of short names are obviously formed after the pattern of expressive formations in (a)ša, uša, usja, unja, ura ²⁾, e.g. Darja:Daša, Grigorij:Griša, Jakov:Jaša, Mar(i)ja:Maša, Anna:Njusja and Njura, Evdokija:Dusja and Dunja, Ljudmila:Ljusja, Agrafena:Gruša and Grunja, Aleksandr:Šura (from Sašura), Aleksej:(A)loša and Ljosja. Now and then the regular short name and that in -ša appear side by side as independent short names: Pava and Paša (from Pavel), Stjopa and Stjoša (from Stepan). Apparently without any connexion with the full name is Tuka mentioned by one of the informants as a short name for Natalja, next to Nata and Talja. Possibly we have to do here with a quite different name. Černyšov has pointed out in his above mentioned article that in different parts of Russia existed the habit to change somebody's name in case of severe illness to ward off evil influences. According to him only this habit may explain the odd formation of names one sees in Butka or Bus'ka for Aksjutka, Šaročka for Susanna, Kantka for Antonin etc.

A few names have as their second syllable nja: Mar(i)ja:Manja, Michail:Minja, Sofja:Sonja. We venture to suggest that they are

²⁾ To be discussed later on.

formed on the analogy of names like Vanja (from Ivan), Danja (from Dani(i)l) and Tanja (from Tatjana).

In some cases the structure of the full name is already in conformance with the pattern of the short name: Vera, Zoja, Ksenja, Nina.

Trisyllabism we found only in two cases, to wit Vladimir:Volodja (but next to this one meets also with Lodja), Sergej:Serjoža.

In five cases the informants were unable to produce a short name of the described pattern, namely in the case of Andrej, Gavri(i)l, Karp, Makar and Nikita.

As regards the function of the short name one might say that its use may bring about a certain degree of familiarity, but does not imply that the persons concerned are on intimate terms.

Let us now consider the formation of expressive names. From the data supplied by the informants fifteen different processes came to light which we divided according to their distribution into two groups: A a group of six processes occurring with great regularity in wellnigh all our names, B a group of nine with more or less restricted distribution. As paradigm for those of group A we have chosen Tolja (from Anatolij). As regards group B other names had to be taken as well on account of its restricted distribution. Thus in the list one will also find Saša (from Aleksandr), Nata (from Natalja), Anja (from Anna), Ljova (from Lev), Valja (from Valentin) and Griša (from Grigorij).

The figures after the B formation refer to the number of cases supplied unanimously by all three informants.

A	B
1 Tolja	8 Toljúška (33)
2 Tol'ka	9 Toljusja (32)
3 Tolečka	10 Sašura (20)
4 Tolen'ka	11 Natunja (17)
5 Toljucha	12 Anjuta (13)
6 Toljuša	13 Tolik (40)
7 Tóljuška	14 Levjonok (10)
	15 Valjok (28)
	16 Grišuk (4)

We distinguish in this series fourteen hypocoristic and only one pejorative name. Let us begin our discussion with the latter. The pejorative name is derived from the short name by adding the suffix -ka

to the root. Further examples are: Kuzja (from Kuz'ma): Kuz'ka, Seva (from Vsevolod): Sevka, Nadja (from Nadežda): Nad'ka, Jara (from Jaroslav): Jarka. As it appears from these examples the final consonant of the root does not undergo any change. As the only exception to this rule we found: Zina (from Zinaida): Zin'ka. The accent is always on the penultimate. Occasionally a pejorative name is also derived directly from the full name: Stepan: Stepanka (next to Stjopka). In case of a full name having several independent short names a pejorative name is derived from each of them, e.g. Ljuda: Ljudka, Ljusja: Ljus'ka and Mila: Milka (all three from Ljudmila).

The pejorative name is a closed formation, i.e. it is not susceptible of further derivation. It may be used to insult, to grieve, to hurt or to belittle the addressed person and in that case is spoken, of course, with a snapping or snarling intonation ³⁾.

However, as we had already occasion to note, this name may also do duty as an endearing name. As a matter of fact children are often addressed continuously with this name, in which case it accordingly takes over the function of the short name.

We now come to hypocoristic names. A further investigation of these soon disclosed that their number is still considerably larger than that of the above list. In the first place, the formal differences we observe in the series Tolja:Tolečka:Tolen'ka recur in the types Toljuša, Toljusja, Sašura, Natunja, Anjuta as follows:

Tolja	Tolečka	Tolen'ka
Toljuša	Toljušečka	Toljušen'ka
Toljusja	Toljusečka	Toljusen'ka
Sašura	Sašurečka	Sašuren'ka
Natunja	Natunečka	
Anjuta	Anjutečka	Anjuten'ka

In every horizontal row we observe the same graduation of expressivity.

A second correlation exists between the pair Tolja:Tolik and the types Toljusja, Sašura, Natunja:

³⁾ In Russian the pejorative name is called *klička*. Mrs T. Voogd-Stojanova called our attention to a curious letter of Belinski to Gogol' where he writes about "strany, gde ljudi sami sebja nazyvajut ne imenami, a kličkami: Van'kami, Vas'kami, Stjoškami, Palaškami".

Tolja	Tolik
Toljusja	Toljusik
Sašura	Sašurik
Natunja	Toljunčik

Finally we found still a third correlation, viz between the pair Jaša:Jašek and the types Anjuša, Žoržik, Levjonok, Varjok:

Jaša	Jašek
Anjuša	Anjušek
Žoržik	Žoržicek
Levjonok	Levjonoček
Varjok	Varjoček

Furthermore the names in -unja, -ura, -uta all three admit suffixation with -ka. It seems, however, doubtful whether we have to consider this as the same suffix-ka as found in pejorative names, as the former never have pejorative value. Frequently, moreover, formations in -urka, -utka seem to be the only ones to occur e.g. Daša:Dašurka, Klava:Klavurka, Vitja:Vitjutka, Maša:Mašutka. However, owing to the rarity of these processes it is extremely difficult to attain certainty in this matter.

Hypocoristic names, just like pejorative ones, sometimes are derived directly from full names, especially those in -en'ka and -uška: Boris: Borisen'ka (next to Boren'ka), Danila:Danilen'ka (next to Danen'ka), Fjodor:Fedoruška (next to Fedjuška), Stepan:Stepanuška (next to Stjopuška). It seems that formations like these are preferably used in addressing children. A few hypocoristic names show the same feature as short names, viz alongside of them there occur abbreviated call names formed by casting off the final vowel: Vovuša:Vovuš, Petrusja: Petrus', Grišunja:Grišun'.

As regards the types Levjonok and Grišuk (B 14, 16) the informants appeared to be far from unanimous. Of the first one two informants gave a rather large number (about half the total number of names), in contradistinction to the third, who considered these forms as exceptional. According to her this type of name is only possible in those rare cases where it corresponds with the well known word-formation reserved for youngs of animals such as Katja:Katjonok. The same informant objected also to the names in -uk of which the others had given a small number (11). Unanimity as to its occurrence

was found only in four cases. Grišuk, according to the last mentioned informant, would have come into being in stead of Grešok to avoid an association with grešok, diminutive form of grech.

The structure of the short name restricts the use of certain names. Thus short names in -ša do not admit the suffixes uša and uška. In certain individual cases sound-correspondences with "normal" words may work prohibitively. Thus Seva (from Vsevolod) and Kapa (from Kapitolina) do not admit the suffix -ucha owing to the existence of the words sivucha (brandy) and kopucha (dawdler).

Final consonants usually do not undergo any change: Seva:Sevon'ka, Lena:Lenon'ka, Žora:Žoročka, Dima:Dimuška, Lida:Lidurka. There are however exceptions, thus hard final consonant becomes soft in Šura:Šuren'ka, Ljuba:Ljuben'ka (next to Ljubon'ka), Stjopa:Stjopen'ka (next to Stjopon'ka), Vera:Verjuška, Kira:Kirjuška, Lara:Larjuška (next to Laruška). Seldom we find the reverse, such as in Tanja:Tanočka (next to Tanečka).

Some of the suffixes have alternants, of which the most important are: -in'ka / en'ka, e.g. Gavril:Gavrin'ka / Gavren'ka; -ička / ečka, e.g. Valja:Valička / Valečka; aša / uša, e.g. Ljuba:Ljubaša / Ljubuša, Dima:Dimaša, Jura:Juraša, Nata:Nataša; -ik / -čik, e.g. Jurik / Jurčik; -ok / ek, e.g. Mašok/Mašek.

As to the accent we observe that of the trisyllables three retain the accent of the short name, viz the types Tolečka, Tolen'ka and Tóljuška, whereas the eight remaining ones shift the accent to the penultimate. Of the bisyllables one retains the accent of the short name, viz Tolik, while the other two are both oxytone.

We must now pass on to the most difficult part of our discussion of hypocoristic names namely the ascertainment of their respective values.

Any attempt to exactly delimitate and classify all the subtle emotional shades conveyed by the different expressive formations we have found would be a hazardous undertaking. Expressive-affective elements like these, fluctuating and elusive as they are, resist attempts at rigid classification. Their expressive value depends to a great extent from the persons concerned and the actual situation of speech and may vary from case to case. We shall confine ourselves to a few remarks.

If we survey the whole gamut of hypocoristic names and compare their respective values, the formations marked by the vowel u appear

to be more expressive, tend to be more intensive, more strongly coloured than the other ones not marked by this vowel, in other words their emotional content to an important extent is determined by this vowel. Turning now first to the formation listed in group A we may confront Tolečka, Tolen'ka as the least and Toljucha, Toljuša, Tóljuška as the most expressive group. As a matter of fact the two first mentioned forms constitute the most normal endearing names. The forms in -en'ka seem to express a higher degree of tenderness or affection than those in -ečka, but in practice there is only slight difference between them. People often use them one after another without, apparently, being conscious, of any distinction. Of the formations in -u the third one, Tóljuška, has, as a rule, the highest intensity. The second, Toljuša, has a friendly and cordial colouring, while the first, Toljucha, occupies, approximately, a medial position between the other two. In a similar way in the B group may be confronted Toljúška, Toljusja, Sašura, Natunja, Anjuta as the forms with the highest degree of expressivity and Tolik, Levjonok, Valjok, Grišuk as the least expressive. The formations of the type Toljúška mostly have an emotional value characterized by the informants as "igrivo", i.e. playful, with an undertone of mockery, sometimes slightly pejorative. As to the formations in -ik and -ok the latter seem to be somewhat more intimate than the former. Thus Vanjok is usually only employed in the intimate sphere of the family, whereas Vanik may also be used by strangers. The same distinction holds good in the case of Jurik and Jurok, Tanik and Tanjok, Vitik and Vitjok.

Finally we shall now consider very briefly the discussed formations with regard to similar derivations from generic names. Comparing the two we see next to formations occurring in both groups also a number which are strictly confined to proper names and only exceptionally appear outside this group, viz those in -ucha, -uša, -usja, -ur(k)a, -unja, -ut(k)a, -uk. It appears, moreover, that in those few cases we have nearly always to do with words referring to kinship relations. We give a few examples partly taken from Belic: djadjuch(a), bratuch(a), matucha, tetjucha, kozlucha, papaša, mamaša, plemjaš, mamuša, mamusja or mamus', dedusja, tetusja, ditjusja. All these have hypocoristic value. It may be worthwhile to note that in the word-class of adjectives there occurs a formation of expressive-affective character which seems more or less to correspond with the last mention-

ed forms, e.g. kisljusen'kaja jagoda, miljusen'kaja kukolka, korotjusen'kaja nožki. These are special forms of very limited usage mostly employed when addressing children. Russian grammars make no mention of them. As to the other formations the following examples may be of interest: dočurka, devčurka, snegurka or sneguročka, kočurka, pečurka, mamunja, dedunja, sestrunja, maljuta, men'sutka, krošutka, porosuk, barčuk, detjuk, pesjuk. Several of these forms seem only to occur in dialects. Now it is well known that words like father, mother, aunt, uncle etc come very close to proper names. There can not be the slightest doubt that children up to a certain age do feel them as such.

Consequently we may say that the seven above mentioned formations are unique, that is to say that they have no counterpart in other wordclass systems ⁴). As proper names appear to have a system of morphologic categories which in its entirety does not occur anywhere else in Russian morphology, our conclusion must be that they are not to be regarded as a special subclass of substantives, a view held up by Karčevskij, Vinogradov and others, but constitute a word class of their own.

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⁴) Compare Uhlenbeck, *The study of wordclasses in Javanese*, *Lingua*, April 1953, p. 349.

ORIGINE DE L'APOPHONIE *e/o* EN INDO-EUROPÉEN

Résumé

L'apophonie *e/o* en indo-européen est le résultat de deux changements phonétiques: 1° d'abord, *e* tonique et protonique > *o* devant voyelle postérieure, ce qui explique les alternances du type φέρω: φόρος, δέρκομαι: δέδορκα, τέμνω: τομή, tandis que les formes du type φορέω sont analogiques; 2° ensuite, *e* posttonique > *o* devant sonante, ce qui explique les alternances du type φέρετε: φέρομεν, πατήρ: πάτωρ, φρήν: ἄφρων, mais il est impossible d'expliquer l'alternance *e/o* dans tous les suffixes et toutes les désinences.

Il y a une dizaine d'années, M. Kuryłowicz a publié, dans *Acta Linguistica*, un article ¹⁾ où il a formulé six principes relatifs aux changements morphologiques. Une de ces formules dit: "Un morphème bipartite tend à s'assimiler un morphème isofonctionnel consistant uniquement en un des deux éléments, c.-à-d. le morphème composé remplace le morphème simple", ce qui, traduit dans un langage plus simple, veut dire que les changements analogiques consistent dans la propagation des alternances, et non pas dans leur suppression, comme on l'admettait communément. Convaincu de la justesse de sa loi, M. Kuryłowicz en a tiré une conclusion logique: l'apophonie *e/o* en indo-européen n'est pas, dans son ensemble, le résultat de changements phonétiques, comme le supposaient tous les spécialistes, pour grandes qu'aient été les différences d'opinion entre eux, mais elle est d'origine presque exclusivement analogique ²⁾. Dans un article de *Lingua* ³⁾, nous avons fait remarquer que, confrontée avec des faits empruntés à l'histoire de deux langues, la loi de M. Kuryłowicz semble fausse et que, si les nombres obtenus pour ces deux langues étaient valables aussi pour les autres, il n'y aurait tout au plus qu'une chance contre quatre pour que cette explication de la genèse de l'apophonie *e/o* en indo-européen soit plausible.

Entre temps, nous avons fait de nouveaux dépouillements, partiels

¹⁾ J. Kuryłowicz, *La nature des procès dits "analogiques"*, *Acta Linguistica*, V (1949), p. 15-37.

²⁾ J. Kuryłowicz, *L'apophonie en indo-européen*, Wrocław, 1956.

³⁾ W. Mańczak, *Tendances générales des changements analogiques*, *Lingua* VII (1958), p. 298-325 et 387-420.

ou complets, de grammaires historiques ou de dictionnaires étymologiques et avons constaté que dans l'histoire de toutes les langues examinées, les changements analogiques consistent plus souvent dans la suppression que dans l'introduction de l'alternance. Voici les chiffres sur lesquels s'appuie cette assertion :

Brockelmann (sémitique)	90%
Dworecki (polonais)	93%
Endzelin (lette)	100%
Ernout (latin)	92%
Gutenbrunner (islandais)	98%
Kuznecov (russe)	83%
Meyer-Lübke (roman)	88%
Nyrop (français)	99%
Simonyi (hongrois)	92%
Smal-Stockyj et Gartner (ukrainien)	94%
Wilmanns (allemand)	67%
Wright (gothique)	100%

En déduisant une moyenne arithmétique de ces chiffres ⁴⁾, on pourrait corriger notre ancienne assertion en disant qu'il n'y a qu'une chance contre dix pour que l'explication de l'origine de l'apophonie *e/o*, proposée par M. Kuryłowicz, soit vraie. Mais en réalité, même une telle correction serait la preuve d'une indulgence excessive envers la théorie de cet auteur : en effet les chiffres présentés ci-dessus nous autorisent à cette seule constatation que, dans les langues examinées, il n'y a en moyenne, sur une dizaine de faits d'abolition de l'alternance, qu'un seul fait d'introduction de l'alternance par voie d'analogie. Mais il est impossible de fermer les yeux sur la différence essentielle entre les faits d'introduction de l'alternance dans les langues

⁴⁾ Notre article de *Lingua* a provoqué, de la part de M. Kuryłowicz, une réponse que l'auteur, qui a l'habitude d'écrire au sujet de problèmes de linguistique générale en français, a rédigé, pour une raison qui nous est inconnue, en polonais (*Bull. de la Soc. Polon. de Ling.* XVII (1958), p. 203-219). Cet article nous a incité à écrire un ouvrage intitulé *Tendances générales du développement morphologique*, qui, outre une réponse à M. Kuryłowicz, présente six nouvelles hypothèses relatives aux changements analogiques, et qui, malheureusement, attend toujours un éditeur. C'est à cet ouvrage que nous empruntons les chiffres mentionnés.

étudiées et le phénomène que M. Kuryłowicz postule pour l'indo-européen. Dans les langues examinées, il ne s'agit que de l'introduction sporadique de l'alternance, limitée à des cas plus ou moins isolés, tandis que l'apophonie en indo-européen, qu'on trouve dans toutes les parties du discours, aussi bien dans les racines que dans les suffixes et les désinences, est un fait de portée générale, vu que la grande majorité de morphèmes à vocalisme *e* présentent aussi le vocalisme *o*. Rien de semblable n'est à signaler dans les langues dont l'histoire nous est connue par des textes. Voici pourquoi il est hors de doute que l'apophonie *e/o*, ou tout au moins la plupart des cas de ce phénomène, ne peut être que d'origine phonétique.

Nous n'arrivons pas à comprendre comment M. Kuryłowicz a pu écrire un gros livre sur l'apophonie en indo-européen, dont la loi citée ci-dessus est une prémisse fondamentale, sans avoir fait aucune tentative de vérifier cette loi sur des matériaux historiques. C'est comme si un architecte faisait élever un gratte-ciel sans se soucier des fondations. Cela est d'autant plus surprenant qu'il ne faut que quelques heures pour compter, dans une grammaire historique ou dans un dictionnaire étymologique, tous les changements analogiques consistant dans la suppression ou dans l'introduction de l'alternance.

Selon Güntert ⁵⁾ et Hirt ⁶⁾, l'apophonie *e/o* serait due à un déplacement de l'accent. Cette explication nous semble aussi peu convaincante que celle de M. Kuryłowicz, et ceci pour la même raison: le déplacement de l'accent n'a provoqué un changement de *e* en *o*, à notre connaissance, dans aucune langue, et il nous semble qu'on n'a pas le droit de supposer, pour l'époque préhistorique, un changement linguistique qui n'a jamais eu lieu à l'époque historique.

A notre avis, les deux règles phonétiques suivantes expliquent l'origine de l'apophonie *e/o*:

I. D'abord, e tonique et protonique devant voyelle postérieure (a, o, u) devient o.

II. Ensuite, e posttonique devant sonante aboutit à o.

Pour commencer par le plus simple, illustrons, avec quelques exemples, la règle II, qui, étant relative à un changement moins éloigné de l'époque historique que le changement dont il est question dans la

⁵⁾ H. Güntert, *Zur o-Abtönung in den indogermanischen Sprachen*, IF XXXVII (1916), p. 1-87.

⁶⁾ H. Hirt, *Indogermanische Grammatik*, II, Heidelberg, 1921, p. 172 suiv.

règle I, a été moins obscurcie par des changements analogiques que la règle précédente.

Le vocalisme *o* apparaît dans les formes où *m* ou *n* existe à l'époque historique, telles λέγομεν, λέγομαι, λεγόμεθα, ἐλέγομεν, ἐλεγόμεν, ἐλεγόμεθα, λέγονται, ἔλεγον, ἐλέγοντο, et dans des formes où l'existence de *m* ou *n* à l'époque préhistorique est vraisemblable, telles λέγω, λέγουσι.

Par contre, *e* posttonique, dans les formes où il n'est pas suivi de sonante, se conserve tel quel: λέγεις, λέγει, λέγετε, etc.

C'est de la même manière que s'explique l'alternance dans les noms comme πατήρ, πατέρες: ἀπάτωρ, ἀπάτορες, skr. *pitārah*: *tvātpitārah*, γενετήρ: γενέτωρ, δωτήρ: δώτωρ, etc. ou la différence entre ποιμήν, λιμήν, ύμήν, σωλήν, φρήν d'une part et τλήμων, ἄκμων, γνώμων, δαίμων, ἄφρων d'autre part.

Passons maintenant à la règle I, en commençant par le verbe. A l'époque qui précède le changement de *e* posttonique en *o* devant sonante, la racine, dans un verbe comme δέркоμαι, se trouve toujours devant *e*, sauf au parfait, ce qui explique pourquoi l'*o* de δέδορκα s'oppose à l'*e* de tous les autres temps, abstraction faite de l'aoriste. Etant donné que le sanskrit présente *dadārça*, on peut admettre que δέδορκα < **dedórka*, que le changement *e* > *o* s'est effectué en position tonique. Seule la 3e pers. sing. δέδορκε fait difficulté. Il faut supposer ou bien que la désinence primitive comportait un vocalisme postérieur ou bien que l'*o* de la racine a été généralisé, par analogie, dans tout le paradigme du parfait.

En ce qui concerne les noms, pour échapper à une tendance propre à tout chercheur et qui consiste à attirer l'attention plutôt sur des faits qui confirment la règle qu'on propose que sur des faits contraires, nous nous bornons à ne citer que ce que Meillet, *Introduction à l'étude comparative des langues indo-européennes*, 8e éd., écrit sur le vocalisme et la place du ton dans les noms:

Thèmes à suffixe zéro. Aucune indication.

Thèmes caractérisés par la voyelle thématique. "Le cas le plus important est celui des thèmes à vocalisme radical **o* et ton sur la racine, indiquant l'action ... Ils apparaissent ... surtout en face de présents radicaux thématiques ... : φόρος-φέρω ... Les mêmes thèmes, avec le ton sur la voyelle thématique, ont souvent le caractère d'adjectifs: τομός, à côté de τόμος ... Des thèmes à vocalisme radical zéro se ren-

contrent notamment au second terme des composés: νεογνός ... Il y a une petite catégorie d'adjectifs à vocalisme radical **e*: le plus remarquable est skr. *sánaḥ* ... Il y a aussi des neutres à vocalisme radical **e* comme gr. *ἑρπυον*."

Suffixe *-*es*-. "Il fournit des noms ... à vocalisme **e* de la racine, ton sur l'élément présuffixal: κλέψος ... A côté des abstraits neutres ayant le ton sur la racine, il y avait des adjectifs ayant le ton sur le suffixe ...: ψευδής à côté de ψεύδος ... Quand la notion désignée par le thème en *-*es*- est conçue comme agissant ..., le suffixe est tonique et offre, au nominatif et à quelques autres formes, le vocalisme **o*: αἰδώς."

Suffixe *-*eu*-. "Avec *-*eu*- sont formés de nombreux adjectifs, ayant le ton sur le suffixe; le vocalisme radical ordinaire est zéro ...: βαρύς ... Le vocalisme radical est **o* dans d'autres cas: πολύς."

Suffixe *-*yo*- (*-*iyo*-). Aucune indication.

Suffixe *-*en*-. Aucune indication.

Suffixe *-*no*-. "Souvent aussi le suffixe *-*no*-, portant le ton, s'ajoute à la forme sans **e* de la racine: skr. *pūrnāḥ* ... Le même suffixe ajouté à la racine (ou plutôt à un thème à suffixe zéro) portant le ton et avec des degrés vocaliques mal définis, mais notamment avec le vocalisme **o*, fournit des noms d'action: lit. *sāpnas* ... Le vocalisme **o* de la racine est normal dans le type grec: *φόργανον*."

Suffixe *-*mo*-. Aucune indication expresse.

Suffixe *-*er*-. Aucune indication.

Suffixe *-*ro*-. "Il s'attache aussi à des racines d'une manière immédiate, et alors le vocalisme radical est **o* ou zéro: got. *bairts* ..., v.h.-a. *bittar* ...; le vocalisme zéro est le plus fréquent."

Suffixe *-*lo*-. Aucune indication.

Suffixe *-*et*- (*-*ed*-). Aucune indication.

Suffixe *-*to*- (*-*do*-). "Le suffixe *-*to*-, portant le ton, s'ajoute à la racine au degré zéro pour former des adjectifs: skr. *ṣrutāḥ* ... Il y a aussi des substantifs en *-*to*- à vocalisme radical **o* et ton sur la racine: φόρτος."

Suffixe *-*ko*-. Aucune indication.

Suffixes *-*yes*- (*-*iyes*-) et *-*istho*-. "La racine a le vocalisme **e* et le ton: skr. *svādīyas*- ... *svādīṣṭhaḥ*.

Suffixe *-*tero*-, *-*toro*-, *-*tro*-. Aucune indication.

Suffixe *-*ter*- (et *-*tel*-). "La racine avait le vocalisme *e* ...; la

place du ton est incertaine et variait sans doute au cours de la flexion: γενετήρ, γενέτωρ."

Suffixes *-tro- (*-tlo-) et *-dhro- (*-dhlo-). "Le vocalisme radical *e ..., le ton sur la racine."

Suffixe *-tei-. "Il sert à former des noms d'action, à vocalisme zéro de la racine; la place du ton variait sans doute au cours de la flexion: skr. *gātīh* et *gatīh*."

Suffixe *-teu-. "A en juger par l'indo-iranien, le vocalisme radical est *e (ou *o), et le ton est sur la racine. Mais quelques mots isolés ont le vocalisme zéro: lat. *portus*, v.h.-a. *fur-t*."

Suffixe *-men-. "Il sert à former des noms d'action ...; ils ont le vocalisme *e de la racine ... et le ton sur l'élément prédésinentiel: φέρμα ... Mais le vocalisme zéro de la racine et le ton sur le suffixe se trouvent aussi ... ὤμην."

Suffixe *-went-. Aucune indication.

Il en résulte que la répartition du vocalisme *e/o* dans les thèmes nominaux est la suivante:

Vocalisme <i>o</i>	Vocalisme <i>e</i>
-o-	-es-
-no-	-yes- (-īyes-) et -istho-
-ro-	-ter-
-to-	-men-
-eu-	-tro- (-tlo-) et -dhro- (-dhlo-)
-teu-	-teu-
	-o-

Sans faire la moindre retouche aux assertions de Meillet, on peut constater a) que dans la plupart des cas, le vocalisme *o* apparaît devant des suffixes à voyelle postérieure et b) que le vocalisme *e* apparaît plutôt devant des suffixes à voyelle antérieure. Mais sans trop tirer la réalité à soi, on peut quand même admettre que l'assertion sous a) est sans exception, supposant que, dans les mots à vocalisme *o*, les suffixes -eu-, -teu- se sont réduits à -u-, -tu- avant le changement *e* > *o* dans la racine. Quant à l'assertion sous b), il faut surtout tenir compte du rapport numérique entre les formations du type ἔργον et celles du type φόρος. En outre, il faut prendre en considération qu'il est inconcevable que l'analogie n'ait pas agi pendant la période qui sépare l'état de langue historique de l'époque du change-

ment $e > o$, époque assez lointaine, vu que des traces de l'apophonie existent dans toutes les langues indo-européennes. De toute façon, il n'y a aucune raison pour exclure à priori la supposition que p.ex. les noms du type λέκτρον pourraient avoir subi une influence du vocalisme de la racine du verbe.

En ce qui concerne les verbes comme φορέω, qui, à l'époque historique, sont sentis comme déverbatifs, on a à choisir entre l'hypothèse que ces verbes ont été primitivement des dénominatifs tirés de noms du type φόρος et l'hypothèse qu'il s'agit de déverbatifs refaits d'après les noms d'action mentionnés, étant donné que, en védique, il existe plusieurs verbes du type *rucáya-*, dont la racine présente le degré zéro.

Reste la question de l'alternance dans le suffixe thématique nominal e/o . Il est évident que pour que o , suivant la règle I, puisse apparaître dans la racine de φόρος, un autre o a dû déjà exister dans la finale de ce mot. Comment donc expliquer o dans $-ος$? Avant de répondre à cette question, faisons une digression sur ce qu'on appelle les lois phonétiques. L'assertion que p.ex. en français l mouillé est devenu, au XIX^e siècle, yod veut dire, évidemment, que la communauté linguistique française a perdu, dans le cours du siècle passé, la capacité de prononcer le phonème dit l mouillé et l'a remplacé, dans tous les mots où il avait existé, par yod . Les lois phonétiques ne sont donc autre chose que des assertions relatives à la perte ou à l'acquisition, par une communauté linguistique, de la faculté de prononcer un phonème. Ce qui est important, c'est que l'état qui suit la perte ou l'acquisition de la capacité de prononcer un phonème peut durer parfois très longtemps. P.ex. on admet pour le polonais que l'assourdissement des consonnes finales s'est accompli, dans cette langue, à l'époque pré littéraire. Bien entendu, la supposition que ce changement a eu lieu p.ex. au XI^e siècle est équivalente à l'assertion que la loi phonétique en question agit, en polonais, depuis presque un millénaire, les Polonais ne sachant toujours pas prononcer des consonnes sonores finales. Or si nous supposons, pour l'indo-européen commun, le passage de e devant voyelle postérieure à o , il est aussi possible que nos ancêtres linguistiques n'aient pas su prononcer e devant une voyelle postérieure pendant plusieurs siècles, ce qui est une période suffisamment longue pour que des changements analogiques puissent arriver.

On est d'accord pour admettre que, en indo-européen, dans les

thèmes en *-o-*, la désinence du nom. sing. n'était que consonantique, tandis que celle du gén. sg. comportait en outre un vocalisme postérieur, et que le ton frappait tantôt la racine, tantôt la finale:

Nom. sing.	<i>*bhér-e-s</i>	<i>*bhŷt-é-s</i>
Gen. sing.	<i>*bhér-e-syo</i>	<i>*bhŷt-é-syo</i>

Suivant notre règle I, *e* n'a pu devenir *o* que dans les formes du type **bhŷt-é-syo*, où il était à la fois sous l'accent et devant une voyelle postérieure. Mais il est impossible d'exclure à priori la possibilité que *o* de **-ó-syo* ait pu pénétrer, par analogie, dans **-é-s*, **-e-s*, **-e-syo* encore à l'époque où nos ancêtres linguistiques ne savaient pas prononcer *e* tonique ou protonique devant voyelle postérieure, ce qui a entraîné le changement phonétique de **bhéros* en **bhóros* > *φόρος*, de **bhérosyo* en **bhórosyo*, etc.

En outre, il nous semble utile d'attirer l'attention sur un fait dont bien des chercheurs semblent ne pas se rendre compte, à savoir que l'évolution phonétique des suffixes et des désinences diffère souvent de celle des racines, le développement spécial des suffixes et des désinences consistant à présenter des réductions de leur volume qu'on ne rencontre pas dans les racines. Voici quelques exemples.

En français, *r* final se maintient normalement: *durum* > *dur*, *cor* > *cœur*, *per* > *par*, *quattuor* > *quatre*, pourtant *r* a disparu dans le suffixe de l'infinitif de la 1^{re} conjugaison: *cantare* > *chanter*. Autrefois, il s'était amui aussi dans d'autres infinitifs: *fini(r)*, *recevoi(r)*, où il a été plus tard rétabli sous l'influence de la graphie. *R* final est tombé également dans le suffixe *-eur* < *-orem*, où on le prononce de nos jours sous l'influence de la graphie; les féminins du type *voyageur* – *voyageuse* témoignent encore aujourd'hui de l'ancienne prononciation de *-eur* identique à celle de *-eux* < *-osum*, parce que c'est sur *paresseux* – *paresseuse* qu'on a formé des féminins analogiques du type *voyageuse*, en face des formes plus régulières du type *défenderesse*, où s'est conservé *-itricem*.

En ce qui concerne les suffixes de l'infinitif, on constate une chute irrégulière de *r* aussi en roumain, où *cantare* > *cînta*, *habere* > *avea*, *dicere* > *zice*, *venire* > *veni*, mais *ferrum* > *fier*.

Il en est de même pour certaines langues slaves, où *-i* est tombé dans les infinitifs, p.ex. polonais *spać*, russe *spat'* < slave commun **sŷpati*, bien que *i* final se maintienne normalement dans ces langues.

La chute de *-i* est aussi irrégulière dans les impératifs polonais du type *idź* < slave comm. **idi*. Et cette chute irrégulière de la désinence de l'impératif en polonais est comparable à un phénomène semblable en latin et en allemand, cf. lat. *dic*, *fac*, all. *geh*.

En français, *b* intervocalique passe normalement à *v*: *faba* > *fève*, *ē* aboutit à *oi*: *tēla* > *toile*, *a* postonique > *e*, de sorte que l'imparfait de *debere* devrait différer des formes qu'on trouve en français:

* <i>devoive</i>	<i>devais</i>
* <i>devoives</i>	<i>devais</i>
* <i>devoive</i>	<i>devait</i>
* <i>devoivent</i>	<i>devaient</i>

Comme on voit, les formes existantes sont plus réduites que les formes attendues. Une réduction exceptionnelle des désinences de l'imparfait a eu lieu aussi dans d'autres langues romanes, p.ex. en espagnol *veniebam* > *venía*, en face de *faba* > *haba*.

En russe, *u* final ne tombe jamais sauf dans la désinence de l'instrumental féminin, les formes du type *golovoj* évinçant celles du type *golovoju*.

En russe, *g* persiste toujours sauf dans la désinence du génitif des adjectifs *-ogo/ego*, où *g* est devenu *v*, l'orthographe restant archaïque.

On voit des réductions irrégulières encore plus grandes quand un mot devient désinence. Il suffit de comparer le conditionnel italien avec la flexion du verbe *avere* au parfait:

<i>canterei</i>	<i>ebbi</i>
<i>cantaresti</i>	<i>avesti</i>
<i>canterebbe</i>	<i>ebbe</i>
<i>canteremmo</i>	<i>avemmo</i>
<i>cantereste</i>	<i>aveste</i>
<i>canterebbero</i>	<i>ebbero</i>

Inutile de multiplier le nombre d'exemples, qui sont vraiment légion, d'autant plus que nous expliquons ailleurs la cause générale du développement phonétique spécial des suffixes et des désinences ⁷⁾. Ce qui est essentiel ici, c'est seulement de constater le fait de réduc-

7) Nous présentons une explication générale de ces changements dans un ouvrage intitulé *La linguistique science exacte*, que nous sommes en train de terminer.

tions extraordinaires dans les suffixes et les désinences, dont beaucoup de linguistes ne se rendent pas compte, tels ceux qui présentent différentes hypothèses pour expliquer pourquoi p.ex. dans la plupart des langues slaves *t* final est tombé aux désinences de la 3e pers.: polon. *pisze*, en face de russe *pišet*, polon. *widzą*, en face de russe *vidjat*. En réalité, il n'y a ici aucun problème: il s'agit tout simplement d'un de ces traitements phonétiques spéciaux qui sont tellement caractéristiques pour les suffixes et les désinences, et il est oiseux de chercher une explication particulière du fait en question.

Il est hors de doute que, si nous ne connaissions pas le latin classique, nous pourrions, à la rigueur, reconstituer les radicaux de la langue latine vulgaire, mais sans connaissance du latin littéraire, nous ne serions jamais en état de reconstituer la flexion romane commune. S'il en est ainsi, ce n'est pas parce que les changements analogiques seraient plus fréquents dans les désinences que dans les radicaux, mais précisément parce que les désinences subissent beaucoup plus souvent un traitement phonétique spécial que les radicaux.

Aucun comparatiste ne doit oublier que l'histoire des langues romanes, qui sont la seule famille linguistique dont la langue mère soit connue par des textes, est une pierre de touche pour la méthode comparative. Si l'on prend en considération qu'il n'y a qu'une période de deux mille ans qui sépare le français du latin et que, sur 42 formes actuelles du verbe *chanter*, il n'y a que 8 (donc moins de 20%) qui n'aient pas subi de changements phonétiques spéciaux ou de changements analogiques, rien d'étonnant que nous n'ayons pas envie de nous aventurer dans l'explication de tous les cas de l'apophonie *e/o* dans les suffixes et les désinences, vu qu'il y a certainement plus de deux mille ans qui séparent le grec de l'indo-européen commun. D'ailleurs il est frappant que tandis que la grande majorité des racines à vocalisme *e* alternent avec des racines à vocalisme *o*, l'apophonie *e/o* est très irrégulière dans les suffixes et les désinences, ce qui ne s'explique que par la loi générale que les suffixes et les désinences subissent souvent un traitement phonétique inconnu aux racines.

Somme toute, nous nous contentons d'essayer de ramener l'explication de la genèse de l'apophonie du type $\delta\epsilon\rho\kappa\omicron\mu\alpha\iota$: $\delta\epsilon\delta\omicron\rho\kappa\alpha$ et $\phi\acute{\epsilon}\rho\omega$: $\phi\acute{o}\rho\omicron\varsigma$ à une formule (I) et l'explication de l'origine de l'apophonie du type $\lambda\acute{\epsilon}\gamma\epsilon\iota\varsigma$: $\lambda\acute{\epsilon}\gamma\omicron\mu\epsilon\nu$ et $\pi\alpha\tau\acute{\eta}\rho$: $\acute{\alpha}\pi\acute{\alpha}\tau\omega\rho$ à une autre formule (II). Bien entendu, nous nous rendons compte que ces règles (surtout la Ire,

qui est relative à une époque plus reculée) souffrent des exceptions, mais il ne faut pas se donner l'illusion de jamais arriver à trouver une formule qui explique tout, et cela pour la simple raison que l'évolution linguistique, aussi bien de nos jours qu'à l'époque préhistorique, est toujours une résultante de changements phonétiques réguliers, de changements phonétiques irréguliers (dans les suffixes et les désinences) et de changements analogiques. Si nous nous sommes quand même décidés à proposer cette nouvelle tentative de résoudre ce vieux problème, c'est que

1° nous croyons expliquer plus de cas et avec moins de formes à astérisque que nos prédécesseurs,

2° nous croyons réaliser le postulat de Baudouin de Courtenay, qui, précurseur dans plus d'un domaine de la linguistique, a exigé, il y a déjà longtemps ⁸, de n'expliquer la genèse de l'apophonie *e/o* que par des changements semblables aux changements arrivés à l'époque historique.

Kraków, Łobzowska 9

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⁸) J. Baudouin de Courtenay, *Urindogermanische Alternanz e/o*, IF IV, 1893, p. 53-57.

EIN PHONEM ODER ZWEI?

Inhaltsangabe

Der Aufsatz, der aus Einzeluntersuchungen von Affrikaten und Diphthongen einiger germanischen Sprachen hervorgegangen ist, versucht eine Rangordnung der bisher in solchen Analysen oft willkürlich angewandten Kriterien zu geben. Es stellt sich heraus, dass die Untersuchung der Morphemstruktur sich auf der syntagmatischen Ebene der Sprache bewegt, die funktionelle Analyse auf der paradigmatischen. Erst die Berücksichtigung beider Ebenen ermöglicht die Segmentation und die Identifikation von Sprachelementen, zwei Prozesse, auf denen jedes Klassifizieren beruht.

Ob man eine Lautverbindung als ein Phonem oder eine Folge von mehreren Phonemen wertet, ist für die Bestimmung des Phoneminventars einer Sprache von erstrangiger Wichtigkeit. Die methodologischen Voraussetzungen solch einer Wertung sind, obwohl über diese Frage mehrfach und eingehend gehandelt worden ist, bisher keineswegs geklärt ¹⁾. In Einzeluntersuchungen, in denen entschieden werden muss, ob es sich um Monophoneme oder Phonemverbindungen handelt, bedient man sich teils wortstruktureller (distributiver) teils funktioneller Kriterien. Meistens gebraucht man sowohl die einen als auch die anderen und zieht gelegentlich auch morphonologische Gesichtspunkte heran. ²⁾.

Die Frage der phonologischen Wertung von Lautverbindungen ist so alt wie die Phonologie selbst. Sie stand im Interesse der Phonologen noch bevor Trubetzkoy's phonologische Synthese erschienen ist. Doch erst hier, in Trubetzkoy's „Grundzügen“ ist der erste Versuch gemacht

¹⁾ Vergl. N. S. Trubetzkoy, *Grundzüge der Phonologie*, TCLP VII, Prague 1939, A. Martinet, *Un ou deux phonèmes?*, Acta Linguistica I (1939), S. 94–103, F. Hintze, *Zur Frage der monophonematischen Wertung*, Studia Linguistica IV (1950), S. 14–24, H. Becker, *Zur monophonematischen Wertung*, Zschft. f. Phon. u. allg. Sprw. VII (1953), S. 256 ff.

²⁾ Vergl. W. Merlingen, *Zur Phonologie der englischen Diphthonge und langen Vokale*, Acta Linguistica VI (1950/51), S. 87–93, D. Gerhard, *Noch einmal die schriftdeutschen Affrikaten*, Zschft. f. Ph. u. allg. Sprw. VI (1952), S. 57–76, A. Martinet, *Occlusives and affricates with reference to some problems of Romance phonology*, Word V (1949), S. 116–122.

worden, das Problem theoretisch erschöpfend zu behandeln. Trubetzkoy stellt seine Anschauungen in sechs Regeln zusammen. Die ersten drei sind phonetischer Art und lauten:

Als Realisation eines Einzelphonems kann eine Lautverbindung nur dann gewertet werden, wenn:

1. ihre Bestandteile in der betreffenden Sprache sich nicht auf zwei Silben verteilen,
2. sie durch eine einheitliche Artikulationsbewegung oder im Wege des allmählichen Abbaus eines Artikulationsgebildes erzeugt wird,
3. ihre Dauer nicht die Dauer der in der betreffenden Sprache vorhandenen anderen Phoneme überschreitet.

Diese Regeln bezeichnet Trubetzkoy als Vorbedingungen. Eine Lautverbindung, die ihnen entspricht, ist „potentiell monophonematisch“. Die endgültige Wertung entscheiden drei weitere Regeln:

4. Die Lautverbindung muss in solchen Stellungen vorkomen, in denen in der betreffenden Sprache Phonemverbindungen nicht zugelassen werden;

5. Die allgemeine Struktur des Phonemsystems muss solch eine Wertung erfordern (es muss durch solch eine Wertung ein Parallelismus im System hergestellt werden);

6. Kein Bestandteil der Lautverbindung darf als kombinatorische Variante irgend eines anderen Phonems gedeutet werden können.

Um die phonologische Wertung einer Lautverbindung zu bestimmen, fordert also Trubetzkoy die Berücksichtigung 1) dreier phonetischer Bedingungen, 2) der Wort- bzw. der Morphemstruktur, 3) der allgemeinen Struktur des Systems und 4) der Funktion, die durch gewisse Eigenschaften der Lautverbindung als Ganzheit oder auch durch seine einzelnen Bestandteile erfüllt wird ³⁾.

Nach Trubetzkoy befassten sich mit dieser Frage vor allem A. Martinet ⁴⁾ und F. Hintze ⁵⁾. Martinet versucht die phonologische

³⁾ Ich glaube diese Regel funktionell und nicht etwa wortstrukturell erklären zu müssen. Trubetzkoy's Unterscheidung der Varianten von den Phonemen beruht auf Berücksichtigung der distinktiven Funktion: „die unvertauschbaren Laute können im Prinzip keine phonologischen (distinktiven) Oppositionen bilden. Da sie niemals in derselben Lautumgebung stehen, können sie niemals als einzige unterscheidende Elemente zweier Wortkörper auftreten“. (Grundz. S. 31).

⁴⁾ Siehe Fussnote 1.

⁵⁾ Siehe Fussnote 1.

Wertung von Lautverbindungen ohne die Berücksichtigung phonetischer Regeln zu lösen. Er sieht Trubetzkoy's 6. Regel als wesentlich an, fügt ihr aber zwei Einschränkungen bei:

1. Wenn durch die Vertauschprobe die monophonematische Wertung in einer Stellung erwiesen wurde, so muss diese Wertung auch in anderen Stellungen, in denen die Vertauschprobe nicht dafür spricht, angenommen werden (falls morphonologische Gründe nicht dagegen sprechen).

2. Die zweite Einschränkung berücksichtigt Trubetzkoy's 5. Regel. Eine Lautverbindung muss als Einzelphonem gewertet werden, wenn das dem Parallelismus im phonologischen System entspricht, auch wenn die Vertauschprobe etwas anderes erweisen sollte.

In seinem Aufsatz vom Jahre 1939 spricht sich Martinet explicite gegen die Notwendigkeit einer Berücksichtigung der Wortstruktur (Trubetzkoy's 4. Regel) aus, obwohl er in demselben Aufsatz (worauf Hintze S. 17 hinweist) in der Wertung von konkreten Einzelfällen wortstrukturelle Kriterien heranzieht. Neuerdings tritt Martinet für die Berücksichtigung der Wortstruktur ein, indem er schreibt: „In a language where no consonantal clusters are to be found except homorganic ones of the [tš] type, the latter are best interpreted as single units“ ⁶⁾.

Die Berücksichtigung der Wortstruktur ist auch der Hauptpunkt, für den Hintze besonders eintritt. Er empfiehlt zunächst, „zu einer eindeutigen Definition der Silben und Wortstruktur einer gegebenen Sprache zu kommen, d.h. festzustellen, ob und welche und wievieltgliedrige konsonantischen Phonemgruppen im An-, In- und Auslaut geduldet werden“ (S. 19). Hintze ist der Meinung, dass im Falle, wo die Lautverbindung durch die Vertauschprobe zerlegbar ist und dieses mit der Silbenstruktur übereinstimmt, eine polyphonematische Wertung anzunehmen ist. Wo sich aber ein Widerspruch zur Silbenstruktur ergäbe, müsse monophonematisch gewertet werden, auch wenn die Vertauschprobe für eine polyphonematische Wertung sprechen würde. Hintze spricht auch von der Notwendigkeit, morphonologische Kriterien zu berücksichtigen und führt ein Beispiel aus dem Kopptischen an, wo obwohl Wortstruktur und Vertauschprobe für eine

⁶⁾ Word V (1949), S. 117.

polyphonematische Wertung sprechen, auf Grund morphonologischer Kriterien monophonematisch zu werten sei.

Die Entwicklung der Anschauungen führt uns, wie wir sehen, wieder zum Ausgangspunkt, den Regeln Trubetzkoy's, zurück. Das Neue im Vergleich mit Trubetzkoy beruht letzten Endes auf dem Vermeiden phonetischer Argumente sowie auf dem, übrigens etwas zaghaften Hinweis auf morphonologische Kriterien. Was weiterhin vermisst wird, ist eine Verhältnisbestimmung aller erwähnten Kriterien zu einander sowie eine Bestimmung ihres Wichtigkeitsgrades für phonologische Wertungen. Wer einen konkreten Einzelfall zu bewerten hat, wird sich immer vor die Frage gestellt sehen: womit ist zu beginnen, in welchen Verhältnis stehen die Kriterien zueinander, welches ist das wichtigste?

Die Anschauungen, die im folgenden dargelegt werden, konnten in Einzeluntersuchungen von Lautverbindungen (bes. Affrikaten und Diphthongen) einiger germanischen Sprachen gewonnen werden ⁷⁾. Da sie auf allgemeineren Voraussetzungen beruhen, glaube ich, dass sie auch für die Wertung von Lautverbindungen anderer Sprachen von Nutzen sein können.

Trotz Martinets Versuch, die Frage monophonematischer Wertung ohne die Zugrundelegung phonetischer Argumente zu lösen, bleibt eine einführende Behandlung der phonetischen Tatsachen eine unumgängliche Vorbedingung jeder phonologischen Wertung. Das ist weniger im Sinne „phonetischer Argumente“ zu verstehen ⁸⁾, als als vorausgehende Sichtung des phonetischen Tatbestandes. Ein Textelement, das artikulatorisch und vor allem akustisch als einheitlich erwiesen ist, kann nie polyphonematisch gewertet werden. Trubetzkoy's phonetische Vorbedingungen stimmen mit den von den meisten Phonetikern berücksichtigten Definitionseigenschaften der Affrikaten und Diphthonge überein. Das würde bedeuten, dass Trubetzkoy

⁷⁾ Vergl. Zur phonologischen Wertung der deutschen Affrikaten und Diphthonge, Zschft. f. Phon. u. allg. Sprwsschft; Bd. 11, S. 49 ff.; Theorie und Praxis der monophonematischen Wertung, Kwartalnik Neofilologiczny, VII/1960, S. 203 ff.; Afrykaty współczesnego języka niemieckiego, Germanica Wratislaviensia, III/1959, S. 175 ff.

⁸⁾ Zur Berechtigung phonetischer Argumente in phonologischen Untersuchungen vergl. F. Hintze, *Zum Verhältnis der sprachlichen Form zur Substanz*, Studia Linguistica III (1949), S. 86–105.

seine Untersuchung mit der Feststellung beginnt, dass eine bestimmte Art von Lautverbindungen vorliegt. Es soll nicht die Schwierigkeit übersehen werden, die Trubetzkoy's drei Vorbedingungen mit sich bringen. Sie sind in Einzeluntersuchungen, wegen ihrer Berücksichtigung subjektiver Kriterien kaum zu gebrauchen. Termini wie „einheitliche Artikulationsbewegung“, „allmählicher Abbau eines Artikulationsgebildes“ (Regel 2) sind für Entscheidungen, um die es sich hier handelt, wertlos. Auch die Forderung, die Lautdauer im Vergleich zur Dauer anderer Laute zu berücksichtigen, (Regel 3) ist praktisch unbrauchbar. Wie sollen wir die Dauer einer Lautverbindung mit der eines anderen Lautes vergleichen, wenn uns die objektive Dauer dieses Lautes unbekannt ist? ⁹⁾ Das Bewusstsein dieser Schwierigkeiten erlaubt jedoch nicht, die Notwendigkeit phonetischer Erwägungen überhaupt zu übersehen. Von Phonemen kann nur gesprochen werden, weil es Laute gibt, und deshalb wird jede phonologische Untersuchung immer von ihnen auszugehen haben.

Die Notwendigkeit distributiver (die Wortstruktur betreffender) Untersuchungen ergibt sich aus dem linearen Charakter des Textes, in dem die Sprachelemente in Anreihungsbeziehungen (syntagmatischen Beziehungen) auftreten ¹⁰⁾. Es geht um die Feststellung, in welchen lautlichen Kontexten die Lautverbindung in einer gegebenen Sprache im Rahmen der Morpheme auftritt. Wichtig ist dabei, sich in der Untersuchung der Lautdistribution auf die kleinsten semantischen Elemente, die Morpheme, zu beschränken ¹¹⁾.

Sprachliche Elemente befinden sich aber nicht nur in syntagmatischen, sondern auch in paradigmatischen (Saussure: assoziativen) Beziehungen. Die ersten sind Beziehungen in praesentia unter den nebeneinander stehenden Elementen. Die zweiten sind Beziehungen

⁹⁾ Verg. O. v. Essen, *Überlange Vokale und gedehnte Konsonanten des Hochdeutschen*, Zschft f. Phon. u. allg. Sprwschft X (1957), S. 239: „Wer jemals Laute auf phonetischen Registrierungen gemessen hat, weiss, wie sehr die von einem Vokal oder auch jedem anderen Laut in Anspruch genommene Dauer schwanken kann, weiss auch, dass ein sogenannter Kurzvokal länger ausfallen kann als ein sogenannter Langvokal. Nicht die Dauer als messbare Grösse, sondern die Dauerunterscheidungen als Mittel der Wortdifferenzierung sind sprachlich von Belang“.

¹⁰⁾ F. de Saussure, *Cours de linguistique générale*, 2 Aufl. Paris 1922.

¹¹⁾ Vergl. F. Hintze, *Bemerkungen zur Methodik phonologischer Untersuchungen der Wortstruktur*, *Studia linguistica* II (1948), S. 37–47.

in absentia, gebildet durch ein gemeinsames Merkmal eines im Text stehenden Elements mit einem Element, das sich ausserhalb des Textes befindet. Die syntagmatische Ebene muss bei der Abgrenzung der nebeneinander stehenden Elemente (der Segmentation) berücksichtigt werden. Auf der paradigmatischen Ebene bewegt sich die Identifikation (Bühler: die abstraktive Relevanz). Jede wissenschaftliche Klassifikation muss sowohl auf der Segmentation als auf der Identifikation beruhen. Ohne Berücksichtigung dieser Prozesse gibt es ganz einfach kein Klassifizieren. In jeder Gesamtheit von Gegenständen oder Tatsachen müssen diese zuerst von einander abgegrenzt und dann nach dem Prinzip der abstraktiven Relevanz mit den zu einer bestimmten Klasse gehörigen Gegenständen und Tatsachen identifiziert werden. Das Gleiche gilt auch für die Phoneme: ihre Realisationen müssen zuerst einmal von den anderen Lauten abgegrenzt und dann nach dem Prinzip der abstraktiven Relevanz identifiziert werden. Dass die Identifikation nur auf Grund ihrer sprachlichen Funktion möglich ist, geht daraus hervor, dass die Phoneme als kleinste Sprachelemente eines unter anderem und vor allem kommunikativen Zwecken dienenden Systems ¹²⁾ irgendwie an dieser Hauptfunktion der Sprache beteiligt sein müssen. Sie besitzen, und das ist oft ausgesprochen worden, selbst keine kommunikative Funktion, dienen aber, ausser dass sie das Material der Morpheme bilden, dazu, Bedeutung habende Elemente, Morpheme, zu unterscheiden.

Daraus folgt die Notwendigkeit funktioneller Untersuchungen für die Wertung von Lautverbindungen. Die Frage lautet: erfüllt die Lautverbindung als untrennbare Ganzheit sprachliche Funktionen (steht sie als Ganzheit in distinktiver Opposition), oder aber eines ihrer Bestandteile?

Somit läge die distributive Analyse auf der syntagmatischen Ebene, die funktionelle auf der paradigmatischen. Diese beiden Analysen sind eng miteinander verbunden. Eine ist die Voraussetzung der anderen. Ohne die distributive Analyse gibt es keine funktionelle, ohne die funktionelle Analyse wiederum kann es keine Entscheidung geben, ob ein Einzelphonem oder eine Phonemverbindung vorliegt. Das Entscheidende ist letzten Endes die funktionelle Untersuchung. Es mag Fälle geben, wo auf Grund distributiver Untersuchungen schon

¹²⁾ Vergl. K. Bühler, *Sprachtheorie*, Jena 1934, S. 42 ff.

gesagt werden kann, ob eine problematische Lautverbindung Einzelphonem oder Phonemverbindung ist. Das trifft dort zu, wo die Sichtung der Lautdistribution ergibt, dass in den Stellungen, in denen diese Lautverbindung auftritt, nur Einzelphoneme stehen (niemals Phonemverbindungen). In diesen Stellungen kann es nämlich keine Morphemopposition geben, in welcher die Lautverbindung auftretend mit anderen Lautverbindungen alternieren könnte (in denen Bestandteile der Lautverbindung distinktive Funktionen erfüllen könnten). Wenn in den Stellungen, in denen sich die problematische Lautverbindung befindet, sonst nur Einzelphoneme zu finden sind, kann es nur Oppositionen geben, in denen diese Lautverbindung als Ganzheit mit Einzelphonemen in distinktiver Opposition steht (etwa Greis: Gruss). Auch in diesen Fällen also ist die Distribution kein endgültiges Kriterium. Sie ist es nur mittelbar insofern sie zeigt, dass die funktionelle Analyse zu diesem oder jenem Ergebnis gelangen wird. Im entgegengesetzten Fall, nämlich dort, wo in den Stellungen der entsprechenden Lautverbindung auch andere Lautverbindungen vorkommen, darf nicht geschlossen werden, dass hier polyphonematisch zu werten sei. Die Lautdistribution zeigt hier nur die Möglichkeit einer polyphonematischen Wertung. Ob es sich wirklich um eine Phonemverbindung handelt, können nur funktionelle Untersuchungen entscheiden. Das Prinzip der phonologischen Unzerlegbarkeit, das aus der Tatsache hervorgeht, dass Phoneme kleinste, weiter unzerlegbare Elemente sind, ist dabei stets im Auge zu behalten.

Es ist interessant festzustellen, dass bei einer im dargelegten Sinne verstandenen Rangstellung der Kriterien, Trubetzkoy's 5. Regel, die vom Parallelismus im phonologischen System spricht, bedeutungslos wird. Es bestätigte sich wiederholt, dass überall dort, wo infolge distributiver und funktioneller Untersuchungen eine Lautverbindung phonologisch gewertet worden ist, dadurch nicht nur ein im System bestehender Parallelismus ungestört blieb, sondern dort, wo er infolge anderer Wertung gestört war, wiederhergestellt wurde (z.B. im System der deutschen Sprache durch die polyphonematische Wertung der Affrikaten und Diphthonge).

Es ergeben sich also für die phonologische Wertung von Lautverbindungen folgende Gesichtspunkte:

1. Die Sichtung des phonetischen Tatbestandes (die Prüfung, ob ein Einzellaut oder eine Lautverbindung vorliegt),

2. Die Untersuchung der Lautdistribution im Rahmen der kleinsten semantischen Elemente (mit der in gewissen Fällen abgeschlossen werden kann),

3. Die entscheidende funktionelle Analyse, in der erwiesen werden muss, ob die Lautverbindung als Einheit, oder auch eins ihrer Elemente distinktive Funktionen erfüllt.

Noch ein Paar Worte zur Berücksichtigung morphonologischer Tatsachen. Sie mögen für gewisse Sprachen vielleicht nicht ohne Bedeutung sein. Doch scheint hier auf alle Fälle konsequent geschieden werden zu müssen zwischen phonologischen und morphologischen Gegebenheiten. Im Slawischen, dem Polnischen z.B. haben wir es mit folgenden Beispielen zu tun : *męka* – die Qual, *męce* – der Qual, *męczy* – er quält (*mɛŋk-a*, *mɛnts-ɛ*, *mɛntʃ-Ź*). Wie sind angesichts der Alternation *k* : *ts* : *tʃ* die Affrikaten zu werten? In polnischen Morphemen gibt es nach akzentuiertem Vokal sowohl drei- als auch viergliedrige Verbindungen (z. B. *martwy*, *wartki*, *warstwa*). Die Frage nach

der Funktion der Affrikaten $\frac{ts}{tʃ}$ in einer Opposition $\frac{mɛnts-}{mɛntʃ-}$ ist jedoch

sinnlos, aus dem einfachen Grunde, weil es sich hier um Vertreter ein und desselben Morphems handelt. So wie Phoneme ihre fakultativen oder kombinatorischen Varianten besitzen, können auch die Morpheme ihre Varianten haben. */mɛŋk-/*, */mɛnts-/*, */mɛntʃ-/* sind verschiedene Varianten ein und desselben Morphems. Das Problem ist somit gar kein phonologisches sondern ein morphologisches und gehört nicht in die Phonologie ¹³⁾. In den germanischen Sprachen sind morphonologische Gesichtspunkte für die Frage der phonologischen Wertung von Lautverbindungen jedenfalls belanglos.

NORBERT MORCINIEC

¹³⁾ Etwas anderes ist, dass es in der polnischen Sprache Oppositionen von verschiedenen Morphemen gibt, in denen Bestandteile der Affrikaten distinktive Funktionen erfüllen, z.B.

<i>czelny</i>	<i>czep</i>	<i>czar</i>	
<i>celny</i>	<i>cep</i>	<i>car</i>	usw.

REVIEWS — COMPTES-RENDUS

T. A. MOLL, P. I. INÈNLIKÈJ, *Čukotsko-russkij slovar'*. Sous la réd. de P. JA. SKORIK. Učpedgiz, Leningrad, 1957, 196 p.; tirage: 2000 ex.

Des trois langues paléosibériennes du Nord, le tchouktche est jusqu'à présent la mieux connue, ce qui nous fait regretter que l'effort de l'Učpedgiz n'ait pas abouti plutôt à un dictionnaire, même modeste, du kamtchadal, moins favorisé – disons même mourant. Mais il en est toujours ainsi: en Sibérie comme au Caucase, l'exploration des très petites langues est négligée au profit des plus grandes; on attend encore des Russes, outre le lexique du kamtch. Ouest moderne, celui de l'iénisseï-ostiak, ceux du tavghi et du samoyède-iénisséen, ceux de la plupart des langues du Daghestan etc. Nous possédions déjà l'excellent dictionnaire *Luoravetlansko-russkij (čukotsko-r.) slovar'*, Učp. 1937 de V. Bogoraz (rédigé par S. Stebnickij) et sa contrepartie *Russko-čukotskij slovar'*, Len. 1941 de P. Skorik, venant après les très insuffisants lexiques allemand-tchouktche de L. Radlov 1861 et russe-tch. de M. Petelin 1898. "Posséder" n'est qu'une façon de parler, car tous les efforts pour se procurer les trois ouvrages, pourtant récents, de Bog., Sk. et Moll restent vains: indispensables au progrès de la linguistique paléosibérienne, ils ne peuvent être consultés que dans les rares bibliothèques qui ont eu la chance de les recevoir, imposant au chercheur un gigantesque travail de copie, – encore celui de Sk. ne m'est-il jamais tombé sous les yeux.

La confrontation de ce nouveau vaste dictionnaire tchouktche-russe (noté ici M) avec celui de Bog. (ici B) montre qu'il s'agit d'une entreprise entièrement neuve – sans tenir compte de Sk., qui m'est inconnu. Malgré l'affirmation de la préface que l'ouvrage est basé en partie sur B, les traductions russes des mots ne coïncident généralement point, étant parfois plus abondantes, parfois plus réduites que dans B. Les mots tch. mêmes ont souvent une autre forme que chez B, ce qui s'explique difficilement. En face de la transcription précise utilisée par Bog. dans *Materialy po izučeniju čukotskago jazyka ...*, St. Pet. 1900 et dans *Chuchee*, Washington 1922, déjà simplifiée dans B, celle

de M est grossière. Le but visé était pratique, non scientifique. Chez B les mots étaient groupés par racines, ici ils suivent strictement l'ordre alphabétique russe. Car l'ouvrage est en cyrillique. Après un éclair d'intelligence au début des années 1930, où les auteurs soviétiques introduisirent pour écrire leurs petites langues l'alphabet latin, une réaction anti-occidentale n'a pas tardé: le grand dictionnaire koriak-russe de G. Korsakov-S. Stebnickij de 1939 est déjà en cyrillique. Croit-on donc là-bas que l'avenir soit à cette écriture?

Outre le lexique même pp. 9–163 et des instructions 3–6, l'ouvrage donne de brèves remarques de phonétique et de graphie 164–6 et les principales flexions grammaticales en tableaux 167–185. B fournissait déjà tout cela. Suit 186–194 – et c'est nouveau – un index alphabétique de la forme médiale, illustrée du "Parfait second", des racines verbales variables, comme en offrait le dict. koriak: ainsi *-npet-/npat-*, Parf. sec. *genpetlin*, renvoient à l'Infinitif *rypetyk*. Il manque par contre l'indispensable liste, que donnait B, des nombreux préfixes et suffixes formatifs: les véritables mots tels que B *egn-/agn-* "ne ... pas", *gemge-/gamga-* "chaque", *peč-* "seulement", *tur-/tor-* "nouveau" etc., qui ont la malchance de n'apparaître que prefixés, sont inconnus de M.

On lit p. 165: "Toutes les voyelles du tchouktche se répartissent en deux groupes", les v. claires et les v. sombres (appelées par M faibles et fortes), et seulement 15 lignes plus bas (166): "La voyelle indéterminée *y* peut s'allier aux deux séries vocaliques". Les mots à *y* de série sombre, p. ex. *gytgyn* "lac", sont indiqués comme dans B par le chiffre 2. Notons *nyčvyn-o* "aimablement", en désaccord avec B ... *-u*. M *e* = *æ* (pendant clair de *a*) et *e* (pendant sombre de *i*). B *ə* est inconnu ici: M a le plus souvent *y* (surtout à l'initiale: *y'lqep*: B *əlqep* "clou", *y'ljo*: B *ə'lo* "de jour" etc., *gečevky*: B *gečewkə* "gaiement", *-lqyl*: B *-lqəl* Destinatif, parfois *e* (*ač'eq*: B *ačəq* "anas fuligula", *ilke*: B *ilkə* "yeux fermés", *in'e*: B *inə* "au matin" etc.). La voyelle *u* cause des surprises: elle est ici parfois notée *y*, surtout auprès de *v* (*y'vequč*: B *uwequč* "mari", *tyv-*: B *tuw-* "parler" etc.), parfois développée en *yv*, *vy*, *yvy* (*nevysqet*: B *neusqet* "femme", *ytyve*: B *ətlua* "neveu", *revymrev*: B *reumreu* "perdrix", *ɟ'yvytgyr* (!): B *ju'tgyr* "époque de la nouvelle lune" etc.), ce qui contredit les données classiques et se rapproche du koriak de Kors.-Steb. De même *ivu-*: B *iu-* "fondre". Cas divers de désaccord vocalique: *pargel* "première glace"

et *qanqolgyn* "crabe" (dont le Pl. *qenqyt* est cert. à rect. *qenqut*) pour B *paargel*, *qaanqolgyn*; au contraire *geev* "mélèze" pour B *geu. eljejvir* "co peau" et *ytri* "ils" pour B *alejvyr, ærry. lyljalgyn* : B *lelalgyn* "œil", *r'yryq* : B *re're'q* "argile", par contre *milger* : B *milgyr* "fusil". *neneny* : B *nenene* "enfant". *gyron* "printemps" et dérivés : B *gron* etc. etc. Les mots tch. ne sont malheureusement pas accentués; en revanche les mots russes le sont.

Parmi les consonnes, B *v* et *w* ne sont pas distingués: M *valy* : B id. "couteau" comme *vapaq* : B *wapaq* "agaric" etc. *w-* (M *v-*) + *o, u* a souvent été entendu aussi *γ* (M *g*): *vut-* ~ *gut-* "attacher" etc. De même *gelm-* ~ *velm-* "se tordre" (B *g*). B *wγ* : M *vj* (noté *v'j*) : *ytlyv'jo-* : B *athuwo-* "neveu". B *rr* : M *tr* : *ytri* : B *ærry* "ils", *-vytryn* : B *-wurryn* "semblable à" etc. La lettre *l* garde sa valeur russe, d'où vaine complication: *li, ly* sont inchangés, mais *la, le, lo, lu* écrits *lja, lje, ljo, lju*, p. ex. *ljajolgyn*, Pl. *ljeljut* (supprimez tous les *j* en lisant!) "barbe". De même *če* est écrit *čje*. *g* note *γ* comme dans B. *q* et *η* sont notés resp. *k'* et *n'*. Pourtant l'apostrophe sert ailleurs : d'abord à séparer une consonne d'un *j* suivant, pour éviter aux Russes la prononciation mouillée (*tym'jo*, lisez *tymjo*, non *ty'mo*! "tué" etc.); ensuite à noter l'occlusive glottale après voyelle (*i'ny* "loup" etc.). Celle-ci, victime du caprice des auteurs, est en outre notée par le *tvërdyj znak* après consonne (*m'emyt* "flèches", *r'ev* "baleine" etc.) mais – troisième notation pour le même son! – par le *mjagkij znak* après *č* et *l* (*č'ječjeη* "le froid", *l'uk* "voir" etc.). L'apostrophe vient dans l'alphabet après toutes les autres lettres; c'est dit p. 3, mais elle est oubliée dans le tableau p. 6. A côté de ces maladresses, l'ouvrage de Moll présente un progrès, important surtout pour le comparatiste : une occlusive glottale a été discernée dans un très grand nombre de mots où Bogoraz l'ignorait : *a'tča-* "attendre", *e'e'val* "molaire", *y'lqep* "clou", *p'onp'on* "champignon", *t'yl-* "être malade", *-t'ul* "morceau", *-l'yn* ~ *-č'yn* adjectifs, noms d'agent pour B *a'tča-*, *ee'wal*, *alqep*, *pompon*, *tyl-*, *-tul*, *-lyn* ~ *-čyn* et le nombre de ces mots à ' négligé autrefois est surprenant. Ainsi rentrent-ils phonétiquement au bercail : en face de kamtch. *pqon-*, *kpon-* et kor. *pyon* un tch. *pom-pon* était incompréhensible: *p'on-p'on* ne l'est plus; kamtch. *x-*, *h-* de *xoptylan* "plein", *hopt* "grand" s'expliquent désormais (tch. M *o'pty-* id.) etc. Quelques inconséquences se remarquent: *gyr'ol'atyn* "lieu de vêlement" mais *gyrol'at-* "passer l'époque du vêl." Cas divers de désaccord consonantique ou autre:

M *pekul* : B *pekul* "couteau de femme", *parte* : B *patte* < **par-te* "omoplates", *jat'jol* (lire *jatjol*!) : B *jajčol* "renard", *yjaa* : B *jaa* "loin" (cf. kor. *yjava-*) – ce qui rappelle les doublets B *iam* ~ *yjam* "pourquoi?", *iorgyn* ~ *yjorgyn* "barre transversale du traîneau" –, *ŋav'jel'o* (lire -*vj-*!) : B *ŋawpe'l* "cousine", *gytka-poĭgyn* : B *gytkaj-p.* "râtelier de fusil", *ŋev'-iny* : B *ŋeu-li'ny* "louve", *qytan*, Pl. *qytanyt* (!) "lieu où se réunit le poisson" prob. erreur, cf. B *qətaṣ* id., kor. *qyteṣ* "barrage à poisson".

Malgré ses dimensions, le livre ne semble point contenir plus de mots que B; env. 8000, disent les auteurs, env. 7500 d'après mon estimation. Certains sont indiqués par un astérisque comme "dialectaux", p. ex. *pintekičgyn* "pain, galette" pour *kavkav* du langage ordinaire. La traduction russe des mots est en général soignée, et souvent des explications entre parenthèses en précisent l'étendue sémantique, p. ex. *romavat-* "chauffer (la maison)" en face de B seulement "chauffer". *ivtun* "rys" (životnoe) c.à.d. "lynx" n'est plus ambigu : B traduisait seul. "rys", qui peut signifier aussi "trot". Quelques traductions pourtant sont incomplètes, p. ex. de noms de plantes -nous n'en possédions déjà guère : B distinguait *ynatelgyn* "hedysarum obscurum", *myjmyj* "polygonum polymorphum", *poṣoqəlgyn* "claytonia acutifolia" etc. : M ne donne plus partout que "racine comestible". On observe quelques cas de désaccord : *gyro-ĭ'elgyn* "le mois tchouktche allant du début d'avril au début de mai" contre B *gro-e'lgyn* "le mois tch. all. du d. de mars au d. d'avril". Le cas de *uttuut* simpl. "arbre" pour B "arbre, forêt, bois (Holz), bâton, mélèze" est étonnant. De très rares et courtes phrases illustrent l'emploi de certaines particules.

Les noms sont cités au "cas absolu" (sujet du v. intr.) et leur thème n'est pas donné séparément (évent. sous la double forme vocalique) comme dans B, mais dans les cas – la majorité – où le Nom. ne coïncide point avec le thème, le Pluriel est cité, d'où le lecteur peut tirer le thème – sauf, est-il dit p. 4, pour les noms en -*n* dont le retranchement est automatique. Quand le thème est phonétiquement très différent du Nom., on peut le trouver sous la forme du Pl. à sa place alphabétique séparée, p. ex. *a'nyt* Pl. de *aqyn* "hameçon", *lilit* Pl. de *ljeljelgyn* "gant". Le Pl. est par ailleurs inutilement donné dans maint cas régulier : *i'nnyṣ'i*, Pl. -*t* "harpon", *ilgytev*, Pl. -*yt* "serviette" etc. Les Pluriels en -*ti/-te* sont toujours donnés : *ilir*, Pl. -*ti* "île" etc. Les homonymes *tyqyl* "tremble" et "hibou" ont un autre Pl. (*tyqylti*) que

chez B (*te'lyt*). Les verbes sont cités à l' Infinitif (-*k*), au lieu du Présent Sg3 chez B qui montrait ainsi immédiatement si le verbe est intr. (-*rkyn*) ou trans. (-*rkynen*), distinction indispensable non marquée ici.

Le lexique nous donne des variantes nouvelles de mots déjà connus: *e'ljučîn* à côté de *a'ljočelgyn* (B seul. *aločelgyn*) "poupée", *gytočgyn* à côté de *gytolgyn* (B id.) "côté, flanc", *ilil* ~ *iliil*, *ilyil* "pluie", *janor* ~ *janot* "avant", *myrmyr* ~ *myrymyr* "chose repoussante", *onon*, *onałgyn* ~ *onoon* "abcès", *vonałgyn* ~ B *wuneun* (qui par contre manque ici) "pomme de pin" etc. Des variantes connues manquent par ailleurs: M seul. *ylpylgyn* pour B *əlpylgyn*, *əlpytlyryn* "joue". M permet de très rares rectifications à B: ainsi *myčut* "suc" est en fait le Pl. de *myču-myč* (thème *myču*). De l'identification par M (*a're-*) de B *are-* "ôter" et *a're-* "re-, maintenir" on ne sait que penser.

Le principal mérite du dictionnaire est de nous fournir des mots nouveaux. Outre des participes-noms d'agent en -*l'yn* (*anŋeljoł'yn* "agité, houleux" etc.), des part. passifs en -*jo* (*a'rejo* "maintenu", *a'qer'jo* "blessé" etc.), des noms d'action ou d'état en -*gyrgyn* (*anŋeljo- gyrgyn* "agitation de la mer" etc.), des adverbes de manière en -*gty* (*gyttagty* "avec ruse" etc.), tous de moindre intérêt, un très grand nombre de dérivés nouveaux de radicaux connus sont fournis: *gyrki-m'jet-* "jouer, folâtrer", *gytiŋy-l'yn* "beau", *gytk'-oje-* "attraper par la patte avec le lasso", *ilg-ev-* "blanchir (intr.)", *ine-ny-gnu-v-* "laisser de reste", *in'y-n* "matin", *ynate-nta-* "aller chercher des racines comestibles", *ny-myŋe-p-qin* "enjoué, facétieux" et *myŋe-p-et-*, *-p-čit-* "plaisanter" (cf. B *mŋi-/mŋe-* "fêter", qui par contre manque ici) etc. etc. Particulièrement intéressantes sont les nouveautés suivantes: *ilgy-l'u* "renne à museau blanc", litt. "visage bl." (qu'on évitera de confondre avec *ylvylju* "r. sauvage"), *jajoč-annaan* "salmo gibbosus à points rouges", l. "poisson-renard", *i'mpy-rguk* (rect. -*gun* car le Pl. est -*gut* ?) "porc", l. "à poils recourbés", *myl-ytt'yn* "chat", l. "chien souple", *myrko-pylvyntyn* "étain, aluminium", l. "métal léger", *ŋavan-ponty* "pancréas, rate", l. "foie féminin", *pekyč'y-r'av* "méduse", l. "baleine claquante/explosante" (c.à.d. lançant son nuage d'encre), *p'onlyryn* "bouton (de vêtement)" < *p'oŋ-p'oŋ* "champignon" (cf. kor. *pyon* "ch. ~ bouton"); dans *j'enk* (Pl. non donné) "poisson aigre" -*nk* inusité fait supposer une erreur: rect. *j'enn* < *j'e-/j'a-* "cru, mouillé" + *ynn-* "poisson"?

Encore plus précieux pour nous sont les mots de radical absolument

nouveau, p. ex. *iku-* dans *iku-miŋkyri* “de toute façon” et *iku-t'er* “un peu”, *iqylq'un* “le petit doigt”, *mej'n'uner* “les extrémités du canot”, *myljač* “suc, confiture, miel”, *mymuri* “graisse de phoque avec la peau” (cf. aïnou *mim* “graisse, chair de poisson”?), *nel-* “pêcher au filet rejetable en arrière”, *nelja-nel* “canard lacustre gris”, *ŋyveč-* “s'éveiller” (cf. kor. *ŋyvač-* id.), *o'tljeq* “otarie” (cf. kor. *ulu-/olo-* id.), *parkal* “petite mouette à tête noire” (*par-kalje* “chien ou renne à taches sombres sur les omoplates” ne vient pas de là, mais de *par-* “omoplate” + *keli-/kalje-* “dessin, peinture”), *potykolgyn* “gros orteil”, *typetyt* “petite vérole”, *tutlik* “courlis” (cf. kamtch. *totočx* id.) etc. etc. Quelques-uns de ces mots nouveaux confirment la parenté ouralienne du tchouktche, magistralement démontrée par K. Bouda (*Die Verwandtschaftsverhältnisse der tshuktschischen Sprachgruppe*, Salamanca 1952 et *Tschuktschisch und Finnisch-ugrisch*, *Lingua* IV : 3, 1955) : *ny-vejmen-gin* “accueillant, hospitalier, flatteur” cf. finn. *vaime-nta-* “apaiser, calmer”, mord. E *ojma-*, M *vajma-* “se calmer”; *pu(u)req* “grand esturgeon” (déjà connu par Raiskij : *puüre*k, dans Radlov, *Mém. Ac. des Sc. St Pet.* 1861) cf. your. *puri*, *pure* “brochet”; *tujke-tuj* “brochet” (cf. kor. *tujqe-tuj* id. jusqu'à ghil. *tuk(k)i*, *tozki* “grand esturgeon”) cf. your. *tujji* “thymalle”; *uruke-* (Nom. *oroka-lgyn*) “éperlan, dorsch” cf. sam.-ostk *norak* “barbeau”.

En revanche nous n'avons que faire des mots “tchouktches” modernes tels que *ponjedjel'nik* “lundi”, *vtornik* “mardi” (et toute la série), *janvar* “janvier” (... etc.) et d'autres, pour lesquels il existait déjà de bons vocables tch., p. ex. *paŋ'evŋyto-y'ljon* “dimanche” (alors pour quoi *voskrjesjeŋe*?), *keli-* “dessiner” (pourquoi *risovat*?), *migčitl'en* “ouvrier” (pourquoi *rabočij*?), *kaljetko-ran* “école” (pourquoi *škola*?) etc. La terminologie politique soviétique ne manque pas : *kommunist*, *rjevolucija*, *-cionjer* etc. On attendait la présence d'un terme moins agressif, *rjespublika* (kor.) – puisqu'il y a *sojuz* “union” – mais il manque. Au demeurant, ces néologismes sont la plupart imprononçables pour des Tchouktches et contraires aux lois de leur phonétique : *vlast* “le pouvoir”, *prjedsjedatjel* “président”, *srjeda* “mercredi”, *dje-kabr* “décembre”, *žurnal* “magazine” etc.! Des mots tels que *pionjer-vožatyj* “chef/ guide de scouts”, *kolxozken pravljénije* “direction du kolxose” sont pour les Tch. dépourvus de sens, puisqu'ils disent “chef, guide” *inentyljevyl'yn*, *inenygjivetyl'yn* ou *inenletkulyn* (B) et “direction” *enančjetkogyr-gyn*.

Si beaucoup de mots nouveaux et intéressants sont fournis par le livre, une grande quantité d'autres, connus par B, y manquent par contre, non seulement des mots spéciaux comme B *ikrer* "cadavre de renne tué par un loup", *mykymyq* "hippocampe", mais aussi des mots importants : *čatam* "lune, mois", *imə* "à proximité l'un de l'autre", *jopyn* "lieu élevé", *makyrgyn* "qui?", *ta'ryn valyn* "moindre", *tuwtuwtu* "parole" etc. etc. On regrette l'absence totale des noms de personnes (B n'en donnait pas non plus mais, le dict. kor. en fournissait abondamment) et celle des noms géographiques (B *čaan* "la baie Čaun", *ekuly-men* "le fl. Kolyma" etc.) – sauf M *ajvan* "Esquimaux", *tannytan* "étranger, Koriak, Européen" qui est presque un nom commun, et *rusil'yn* "Russe" que les auteurs ont eu garde de n'oublier! – en partic. il est inconcevable que manque précisément *lu-orawetlan* "Tchouktche" litt. "véritable homme". Voudrait-on faire oublier à celui-ci son nom séculaire? Car on trouve par contre, incidemment sub vocabulo *okrug*, un *čukotkaken*, forgé par les auteurs! Je découvre par ailleurs, dans le titre d'un livret tch. paru en 1959, le nom sous la forme archaïque *lyg'oravetl'a-*.

Des raisons idéologiques ont amené la suppression de tous les termes religieux : B *eŋeŋ* "esprit de chamane, dieu, icône, croix" (et dérivés : *aŋaŋ-ran* "église" etc.), *kele* "mauvais esprit, esprit" (plus d'une demi-colonne de dérivés dans B!), *kamak* "mauvais esprit", *ujwel* "sortilège, maléfice" etc. On a par contre conservé *eŋeŋytkuk* "(B) prier", dont le sens a été déformé en "fêter", de même *eŋeŋytkun* "(B) dieu" déformé en "fête", et *eŋeŋyl'yn* "chamane", mot important à conserver afin de pouvoir ridiculiser et injurier le personnage. Une pudeur aussi ascientifique a fait bannir également les noms des organes génitaux : B *əlqy*, *loločgyn*, *tyrkylgyn*, *ququŋen*, *aŋŋelqy* etc. ainsi que les termes *qujmu-*, *ny-qučun-qin* etc., appartenant à la même sphère, tout comme les termes de déjections : *vyl'i'ŋ*, *a'ttyt*, *e'le'l*, *reqokalgyn*, *eče-*, *a'lajo-* etc. Proprement stupide est la suppression du mot important *e'le'l* qui a, outre le sens d' "excrément", celui fort innocent de "boue, saleté". On ne permet plus aux Tchouktches de jouer aux cartes (cf. les abécédaires moralisateurs!): B *kočyr*, *kočyratko-* ont été proscrits; en revanche leur vice de chiquer (M *reljup*, *reljupyk*) n'a pas été inquiété!

Nous regretterons donc que l'idéologie et la pudibonderie aient été mêlées à une entreprise qui devait être scientifique. Le tableau du vocabulaire tchouktche présenté là est volontairement incomplet, am-

puté, parfois violenté. Pourtant le dictionnaire de Moll et Inènikèj constitue indéniablement un progrès dans notre connaissance, déjà excellente, du tchouktche. Composé pour des besoins pratiques, c'est paradoxalement le linguiste, à condition qu'il possède déjà l'oeuvre de Bogoraz, qui profite le plus: les données nouvelles, très nombreuses, viennent heureusement compléter et parfois rectifier cette dernière.

Pour un ouvrage si étendu, les erreurs qu'on relève sont peu nombreuses. P. 27, col. a, ligne 11 *velgyjeg* rectifiez en *velvy-*, 45 a 19 *itqyjev* rect. *-vyk*, 54 b 17 *konqonalgyn* rect. *-qonna-*, 78 b 2 *noogyrgyn* rect. *noovy-*, ib. 7 *nool'yt* tect. *-l'yn*, 84 a 41 et 44 *nyčiv'ev* et *nyčivqin* rect. *nyvič-*, 88 b 31 *nypytlin'kin* rect. *-nqin*, 106 b 8 *risti* rect. *raisti*, 127 b 35 *taqravytrynran* rect. *-ynran*, 137 a 7 *tejkyjgut* rect. *-jgu*; 40 a 29 *iqylq'un* "le petit doigt", mot nouveau, est écrit 162 a 7 *-lk'un*: quelle forme est la bonne?; 43 b 19 *inengynuvetyk*, s'il est une variante de ce mot cité 42 a, est à lire ici *inenygnu-*; 88 b 11 *nyppyl'ev*, traduit "être petit", signifie en réalité "peu" (adv.). Des mots ne sont pas à leur place alphabétique: 20 b 1 *vanek*, 40 a 21 *ivmytvik*, 42 b 39 *inengevjiljuvyk*, 43 b 27 ss. trois mots en *ineng-*, 75 a 9 *lynkymljan*, 79 a 3 *mumket*, 121 b 32 ss. deux mots en *ryryr-* et deux en *ryrym-*, ib. 42 *ryretljav'jo*, 138 b 27 *ten'ympen'nyk*, 149 b 36 *ytčok*, 150 b 33 *y'tt'yjutil'et*; 69 a 27 et 29 *qysqyčetyk* et *-četyl'yn* sont soit mal rangés soit à rect. *qyčq-*. Parfois l'on trouve le même vocable en deux endroits: *v'ajmagny* (aussi 23 b 7), *rygživetyk* (aussi 114 a 3), *ryrymek* (aussi 121 b 12 bas). Autres inconsistencies: le part. passif de *lyn'nyk* "tenir pour" est écrit tantôt *lyn'jo* (21 b 17, 26 b 8 bas, 27 b 23) tantôt *lyn'jo* (ailleurs); 41 a 17 *im'učit* "esp. de bottes" réfère à une variante *i'mučit* qu'on ne trouve point, de même 98 a 32 *pajvanyrgyn* "dégout"; *otrjad* "détachement", cité sous *pionjer*, est absent du lexique, de même *čukotkaken* "tchouktche" et *nacional'ny-okrug* "district national" sous *okrug*; il existe *nev-učitjel* "professeur femme" mais pas *učitjel**, de même *ta-paroxod-nygyrgyn* "construction de navires" mais pas *paroxod**.

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BJÖRN COLLINDER, *Uralo-jukagirische Nachlese*, Uppsala Universitets Årsskrift. Uppsala (A.-B. Lundequistska Bokhandeln), Wiesbaden (Otto Harrassowitz), 1957, 12, pp. 105–130.

C'est seulement lors de la parution de mon *Plaidoyer pour le youkaghir, branche orientale de la famille ouralienne*, *Lingua* VIII: 4, pp. 403–423, Amsterdam 1959 – où je prie le lecteur de se reporter – que je pris connaissance de cet important article du fameux ouraliste suédois, ce qui explique que je ne l'aie pas mentionné p. 404 ni dans la bibliographie. Ce travail a un double mérite: il précise la situation du youkaghir, ouralien, en face de l'altaïque – soupçonné depuis fort longtemps d'être apparenté à ce dernier –, et fournit une quantité appréciable de rapprochements lexicaux nouveaux – les derniers en date étant alors de J. Angere, UJF, Stockholm 1956, v. *Orbis* VIII: 2, 550–2, Louvain 1959 et ZMDG CIX 439–42, Wiesbaden 1959.

Sur les 72 équations lexicales données, pp. 113–127, 54 sont reprises de Paasonen, Lewy, Bouda, Angere ou Collinder lui-même (JU, 1940, en partie redonnées dans *Fenno-ugric Vocabulary*, Uppsala 1955, auxquelles s'ajoutaient 5 nouvelles dans *Survey of the Uralic Languages*, 518–526, Uppsala 1956). Les 18 autres équations étaient nouvelles: 6 d'entre elles coïncident avec les miennes dans *Plaidoyer* ... : p. 121 *nodo* "oiseau", 122 N *ńoržen* "lichen" (lp. ? ~ your.), omok *nema* "lièvre", 124 oao- "dérober", N *ũje* "aile", 125 ožũ "racine".

Qu'il me soit permis de faire sur les équations les remarques suivantes. P. 114 N *wal* (et non *wal**, erreur d'Angere, *Juk.-deutsches Wb.*, 248!) "à côté de" (et avec lui S *al-de-ge* etc. *Survey* ... 518) n'appartient pas à S *ā* "sous" (N aussi *ā*). P. 115 dans *pige* "marmite" ~ *munbuge* "m. en fer" il n'y a pas d'alternance *i* ~ *u*, mais assimilation à *u*: **lu(du)n-pige* > *mun-buge*, de même dans r. *libo* > youk. **libu* > *lubu* "ou". P. 116 N *eñe* "mère" est emprunté, comme les autres noms de parenté, au toungouse, donc il faut abandonner lp. *ædne* etc. L'étymologie om. *ku* "neige" < our. **kowa* (hgr. *hó* etc.) serait excellente, si la lecture *ku* était assurée: mais il se peut qu'il faille rectifier *ču** v. UDO No. 82. P. 117 *eži* "vivant" ne va pas avec *ežie*, *ižie* "même" (déjà ainsi dans JU 73), mais avec *en-* "vivre", Gér. *ežut* etc. P. 118 om. *tkit* "deux" n'est qu'une erreur pour *kit*, forme attestée (UDO No 26 et p. 80 bas); il ne saurait être question de "vordere Variante von *k*", qui était l'opinion d'Angere, UJF 72. P. 120 *kiŋe* "couteau"

n'est pas youk., mais omok (UDO N° 109): l'erreur vient d'Angere, UJF 49, qui n'a pas distingué là youk., tchouvane et omok. P. 122 N *noržek* (que l'auteur écrit avec *n* ..., encore une faute d'Angere, *Juk.-d. Wb.* 184!) "mousse" est le Nom.-Acc. "emphatique" (*-k*): le mot est *noržen* (Krejnovič). "Peuplier rouge" n'est pas *nolu**, mais *nolut* cf. Bill. *nollod*, Rai. *nolud*, Suv. *nolut*, Joch. The Yuk. 132 *nolut*- (et rect. avec *ɹ!*, *Mater.* 106.⁵⁵); ce mot, comme tant d'autres, manque dans le dictionnaire d'Angere. *nū-* "trouver" n'est qu'un des aspects du radical *nug-* (v. Orbis VIII: 2, 548 bas, ZDMG CX, 86 mil.); ce n'est d'ailleurs pas à mon avis ce mot qui convient à fi. *nouta-*, your. *nææda-* etc., mais youk. *noudi-* (*~ nogdi-*) "épier, guetter, surveiller", dont l'identité avec l'ouralien est évidente. Pour rattacher om. *nema* (lire *ñema**?) "lièvre" à our. **ñoma*, point n'est besoin de rectifier om. *ñoma**: l'alternance *e ~ o* est générale en youk. et om. *e ~ youk. o* est particulièrement attesté (UDO p. 102.⁶). P. 126 *manjaɹo* "ventre" n'est pas youks S ou N, mais tchouvane (et rect. *ɹ!*), de même p. 127 *totak* "cent".

La chute des sifflantes en youk. (our. **sula-* > youk. *alā-*, **sala-* > *oɹo-* etc.), que j'ai établie dans *Plaidoyer* ... p. 420 par de nombreux exemples, est déjà constatée ici p. 124 par Collinder (et même Bouda en 1940: **soma-* > *omo-* FUSSJ 74). Par ailleurs l'auteur abandonne p. 123 l'excellent rapprochement **soŋe-* > *šog-* "entrer", à cause de l'initiale; il suffit pourtant de poser la variante **šoŋe-*, ce qui n'a rien d'exagéré étant donné l'extrême confusion qui règne dans la répartition des sifflantes-chuintantes ouraliennes et les interférences qu'on constate réellement s'être produites.

Dans la première partie du travail, Collinder rappelle les principales équations morphologiques établies par lui en 1940 et montre entre autres pp. 110-2 que beaucoup d'entres elles, au moins dans la flexion du nom, ont un correspondant en altaïque; dans la flexion verbale, seul l'Impératif *-k* trouve là-bas un écho (toung. *-gā*, mo. *-γai* etc.). On constate cependant que, même en expliquant la ressemblance des suffixes flexionnels altaïques avec ceux de l'ouralien par la parenté et non l'emprunt (ce qui est proprement impossible) ou le hasard, ceux du youk. se tiennent bien plus près de leurs correspondants our. – tant par la forme que par l'emploi – que de leurs échos alt. A cet égard, on ne saurait assez insister sur la concordance parfaite des suffixes des cas locaux en samoyède et en youk. (*Plaid.* 406-7). L'auteur a par-

faitement raison de dire (p. 111–2) "... komme ich zu dem Ergebnis, dass die altaischen Sprachen entferntere Verwandte der uralischen sind als das Jukagirische. Für eine nähere Verwandtschaft zwischen Uralisch und Jukagirisch als zwischen Altaisch und Jukagirisch spricht der Umstand, dass die uralisch-jukagirischen Übereinstimmungen teilweise komplex sind".

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PAUL, L.-M. SERRUYS, C.I.C.M., *The Chinese dialects of Han time according to Fang-yen* (University of California publications in East Asiatic Philology, vol. II). University of California Press, Berkeley and Los Angeles, 1959. xix + 350 pp.; 18 sketchmaps in the text, 1 folding map; in-8°. US \$6,00 ¹).

The *Fang-yen*, "the speech of the (different) regions", is as the title indicates, a work on dialects, i.e. dialects spoken in the Han empire at the beginning of the Christian era. The work is traditionally attributed to Yang Hsiung (53 B.C. – A.D. 18) who is also known for his poetry and his philosophical writings; the author of the present work intends to publish separately his researches concerning the correctness of this attribution.

In the present volume the author subjects the contents of the *Fang-yen* to a close study, but before he starts on his primary subject, he reopens the discussion on the reconstruction of the earlier stages of the Chinese language.

During many centuries Chinese scholars had already shown a profound interest in these earlier stages, but due to the non-phonetic nature of the Chinese script they had been unable to arrive at actual reconstructions.

After several attempts by western scholars among whom the French

¹) Earlier reviews of this work in orientalist periodicals were published by Fang-kuei Li, *Journal of the American Oriental Society* LXXIX (1959), pp. 309–310, by G. B. Downer in the *Bulletin of the School of Oriental and African Studies* XXIII (1960), pp. 165–167, and by M. J. Künstler in *T'oung Pao* XLVIII (1960).

scholar Henri Maspero ²⁾ and our compatriot S. H. Schaank ³⁾ deserve honorable mention, it was the Swedish sinologist Bernhard Karlgren who successfully achieved the herculean task of reconstructing first 'Ancient Chinese' of the early seventh century A.D. ⁴⁾ and a number of years later that of 'Archaic Chinese' of approximately the seventh century B.C. ⁵⁾.

Since then an ever growing number of Chinese, Japanese and western scholars have been at work to amplify his results. It may be said in a general way that Karlgren's reconstruction of the language of the seventh century of our era has stood relatively firm ⁶⁾; this is undoubtedly due to the abundance of the material available (the modern dialects; Chinese loanwords in Japanese, Korean and the Indo-Chinese languages; Chinese transcriptions of foreign words, mostly Sanscrit; the indications provided by the early Chinese dictionaries arranged according to rhyme). However, his reconstruction of Archaic Chinese is built on less copious material; moreover, it had perforce to use the reconstructed Ancient Chinese as one of its starting points. In the main, the primary material for this reconstruction consists of a) the rhymes of the Book of Odes and b) the construction of those Chinese characters where unrelated but phonetically similar words are written with the same signs, amplified by different meaning-indicators.

²⁾ "Le dialecte de Tch'ang-ngan sous les T'ang", *Bulletin de l'école française d'Extrême Orient* XX (1920), pp. 1-124.

³⁾ "Ancient Chinese Phonetics", *T'oung Pao*, 1st Series, VIII (1897), pp. 361-377, 457-486; IX (1898), pp. 28-57; New Series III (1902), pp. 106-108.

⁴⁾ *Etudes sur la phonologie chinoise* (Leiden, Stockholm, 1915-1926), popularised in *Philology and Ancient China* (Oslo; 1926).

⁵⁾ "Shi-king researches" and "Word families in Chinese", *Bulletin of the Museum of Far Eastern Antiquities* (Stockholm), resp. IV (1932), pp. 117-185, and V (1934), pp. 1-120, amplified and systematized in *Grammata serica* (1940), of which a new version, *Grammata serica recensa* was included in *BMFEA* XXIX (1957). The results of these later researches were popularized in *The Chinese Language* (New York, 1949). - A very brief survey of the development of the studies in this field is to be found in S. E. Martin, *The phonemes of Ancient Chinese*, Supplement 16 to the *Journal of the American Oriental Society* (1953), pp. 1-7.

⁶⁾ One of the most serious attacks on this reconstruction was published recently by Wang Lien-tseng, "Un dictionnaire phonologique des T'ang", *T'oung Pao* XLV (1957), pp. 51-151, which, however, mostly affects Karlgren's reconstruction of the vowels, leaving the consonantal structure practically untouched.

This system of constructing signs by means of a "phonetic element" and a class-sign, i.e. the system called *hsieh-sheng*, Karlgren assumed to have arisen in about the same period as when the Odes were written, i.e. towards the middle of the first millennium B.C.

Now the first part of the book under review consists of a renewed study of both the rhymes of the Odes and the *hsieh-sheng* system. On the one hand the author concludes that the uniform rhymes of the Odes point to a time later than the 1st century A.D. (p. 21). On the other, his reconstructions based on the *hsieh-sheng* (of approximately the same time, p. 53 ff.) differ considerably from those by Karlgren, as the author, following i.a. P. A. Boodberg ⁷⁾, in his reconstructions insists on accounting for all the phonetic indications provided by the characters written with the same "phonetic element".

In this way he arrives at consonant-clusters both at the beginning and at the end of the monosyllabic word in a manner which Karlgren had tried to avoid (pp. 44-43, cf. p. 103 ff.). It is on this point that the author has been most seriously attacked by the reviewers mentioned in note 1.

The second part of the book (pp. 73-end) is devoted to a discussion of the dialects spoken in the Chinese empire as these appear in the *Fang-yen*. On the localisations provided by this text the author has established dialect areas (pp. 74-101) delimited on the folding map at the end of the volume. By means of statistical computations he then discusses the relationship between the dialects mutually and the position of the standard language of those days (pp. 164-193). Of great interest and permanent value is the final part of the book where the author adduces extra-lingual material, both from history and archeology, which confirms the dialect areas he had established solely on the indications provided by the *Fang-yen*, and which even partly explains otherwise inexplicable relationships (pp. 195-253). The present reviewer can only agree with the author's earlier critic, Professor Fang-kuei Li, that "in all these sections Serruys is more or less free from his preoccupation with etymologies, Pre-Archaic reconstructions, origins of binoms, etc." and therefore "he is at least on

⁷⁾ See e.g. P. A. Boodberg, "Some proleptical remarks on the evolution of Archaic Chinese", *Harvard Journal of Asiatic Studies* II (1937), pp. 329-372; but cf. N. C. Bodman, *A linguistic study of the Shih-ming* (Cambridge, Mass.; 1954), pp. 40-65.

solid ground”⁸⁾. This does not apply to the disquisition on the binoms where the author further expands his methods in the reconstruction of ancient Chinese (p. 103 ff.). In this respect the ideas developed by J. Chmielewski⁹⁾ seem more acceptable.

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M. F. GALIANO, *Diecisiete tablillas micénicas*. Consejo superior de investigaciones científicas, Madrid 1959 (= Serie de Textos de Estudios Clásicos supl. no. 5, p. 111–228). Pr. 75 ptas.

Ceux qui veulent s’initier à l’étude des textes mycéniens, trouveront une bonne introduction dans le livre de M.G., qui, disons-le tout de suite, offre aussi aux spécialistes parfois des remarques intéressantes. M.G. donne d’abord une étude concise du système graphique du linéaire B, des idéogrammes, des règles de transcription, de la grammaire du dialecte mycénien et de sa position parmi les dialectes grecs. Ensuite, il fournit une bibliographie. La principale partie de l’ouvrage consiste dans l’étude des textes de dix-sept tablettes intéressantes, que M.G. traduit et commente; ce sont: Pylos Aa 792, Ab 379, Ad 676, Ae 264, 134, An 207, Ea 800, Eb 416, Er 312, Tn 316, Ta 641, 713, 722, et Cnossos Ai 63, Gg 702, V 52, Sd 0401. Il ajoute une liste de mots importants qui ne figurent pas sur les dix-sept tablettes. Il termine son ouvrage par quelques observations sur le livre de M. Lejeune, *Mémoires de philologie mycénienne*, I (Paris, 1958), et sur d’autres publications qu’il ne pouvait pas encore utiliser pendant la rédaction de son travail. Il est dommage qu’un index des mots étudiés fasse défaut. M.G. a l’habitude de donner à propos de chaque problème toutes les hypothèses qu’on a faites pour l’expliquer; la seule objection que l’on puisse faire à M.G., c’est que, sans doute par un excès de gentillesse envers ses collègues, il ne distingue pas suffisamment les solutions probables de celles qui sont purement possibles ou même hasardeuses et improbables. Il mentionne (p. 228) p. ex., sans la contredire, l’interprétation que donne V. Pisani du début de PY Ta 641: M. Pisani commence par

⁸⁾ p. 310, in the review quoted in note 1.

⁹⁾ “Remarques sur le problème des mots dissyllabiques en chinois archaïque”, *Mélanges publiés par l’Institut des hautes études chinoises* (Paris, 1957), pp. 423–445.

lire d'autres caractères que ceux que la photographie de la tablette présente, il les place dans un ordre insolite, s'efforce d'imposer au texte un ensemble de formes factices et hétérogènes du point de vue linguistique, pour en tirer enfin des conclusions concernant la nature et l'origine du mycénien qui confirmeraient ses propres théories de l'évolution des dialectes grecs.

Il va de soi qu'il y a beaucoup de remarques à faire à propos de tout ouvrage touchant les textes mycéniens. Limitons-nous, toutefois, à deux problèmes d'ordre général: M.G. (p. 120) dit que pour rendre une occlusive devant consonne dans l'écriture syllabique, on lui donne la voyelle de la syllabe suivante (*ku-ru-so* χρυσός), celle de la syllabe précédente (*wa-na-ka-te-ro* Φανάκτηρος) ou une voyelle quelconque (*ru-ki-to* Λύκτος). En réalité, le système graphique est plus rigoureux: on utilise régulièrement la voyelle suivante, sauf naturellement à la fin du mot où l'on se sert de la voyelle précédente (*wa-na-ka* Φάνακς). Les graphies exceptionnelles s'expliquent par l'analogie: *wa-na-ka-te-ro* d'après *wa-na-ka*, *ru-ki-to* d'après *ru-ki-ti-jo* Λύκτιος. — Comme la plupart des mycénologues, M.G. (p. 121, al.) suppose qu'en mycénien, toutes les sifflantes géminées étaient encore intactes, en transcrivant *e-ke-si* ἔγχεσσι (< -es-si), *to-so* τόσσον (<* tot-yo-), *pa-sa-ro* πασσάλω (<* pak-y-). Or, on sait que l'arcado-cypriote, descendant du mycénien, distingue toujours le traitement -σ- < -ss-, < -ts- ou < -ty- (p. ex. *Ἑσέστοι*, φυγάσι, ὄσον) en face du traitement -σσ- < -ky- (p. ex. ἡσσον); la même distinction existe en ionien-attique (l'attique présente -ττ- au lieu de -σσ-: ἡττον). Ceci s'explique par le fait que, dans ces dialectes, -ky- a seulement abouti à -σσ- à une époque où l'ancien -ss- (original ou issu de -ts-, -ty-) s'était déjà simplifié en -σ-:

$$ty > ts > ss > \sigma$$

$$ky > ty > ts > \sigma\sigma \text{ (att. } \tau\tau\text{)}.$$

Par conséquent, il est impossible que d'une part, les formes ἔγχεσσι, τόσσον, d'autre part πασσάλω aient coexisté en mycénien. Ou bien, il faut opposer ἔγχεσι, τόσσον à πασσάλω ou bien, le résultat de -ky- était encore intermédiaire entre -ts- et -σσ-. La dernière hypothèse est plus probable: parfois, on utilise la graphie avec z- pour le résultat de -ky- comme pour celui de -gy-: *ka-zo-e* χάσσοες ou plutôt *κάτ^sσοες* (<* kak-yos-es) comme *me-zo-e* μέζσοες (<* meg-yos-es); la divergence du traitement attique -ττ- et du traitement ionien -σσ- s'explique facilement si

on la place après la période de l'unité ionienne-attique, donc après l'époque mycénienne. La question des sifflantes a été étudiée récemment par E.P. Hamp, *Assibilated clusters in Proto-Greek*, Glotta 38 (1960), 187-190; les faits mycéniens ont été examinés par M. Lejeune, *Les sifflantes fortes du mycénien*, Minos 6 (1958, paru en 1960), 87-137.

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B. TH. KOPPERS, *Negative conditional sentences in Greek and some other Indo-European languages* (Diss. Utrecht, Drukk. Westerbaan). The Hague, 1959, 133 p.

Mlle Koppers étudie le problème de la différence entre $\epsilon\iota \mu\acute{\eta}$ et $\epsilon\iota \omicron\upsilon$, déjà posé par son maître, M. J. Gonda (*The character of the Indo-European moods*, Wiesbaden, 1956). Elle compare les données du grec avec celles du latin, du vieil indien, et, d'autre part, avec celles des langues modernes: elle croit que l'usage du néerlandais ou de l'anglais actuels pourrait éclaircir les subtilités de l'usage des langues anciennes, c'est-à-dire, elle pense que, pour expliquer l'emploi de $\omicron\upsilon$ et de $\mu\acute{\eta}$, il faut se mettre sur le plan psychologique sans se fonder sur les distinctions formelles, les règles grammaticales de la langue, qu' elle attribue souvent à la phantasie des grammairiens.

Elle examine d'abord la conjonction $\epsilon\iota$, en rejetant la théorie que les propositions conditionnelles soient issues d'une parataxe. Elle prend $\epsilon\iota$ pour le locatif du thème démonstratif *e-/o-*; la valeur originale aurait été 'with regard to this'; le suffixe *-i* du locatif serait d'ailleurs apparenté à la particule déictique *-i*. Ensuite, elle étudie la formule $\epsilon\iota \delta' \acute{\alpha}\gamma\epsilon$ introduisant un impératif etc.; elle conclut que $\epsilon\iota$ se rapporte à la situation décrite auparavant, c'est-à-dire que $\epsilon\iota$ a eu la même fonction que la particule $\omicron\upsilon\nu$. Puis, elle aborde les négations; le grec utiliserait $\omicron\upsilon$ pour porter l'attention surtout sur le mot nié, tandis que dans le cas de $\mu\acute{\eta}$, c'est la négation elle-même qui recevrait un accent emphatique. Elle suppose qu' à chaque négation correspond un geste, un mouvement de tête différent; dans les langues modernes, $\mu\acute{\eta}$ devrait être traduit par une négation accentuée. Elle croit retrouver la même opposition dans $\epsilon\iota \omicron\upsilon/\epsilon\iota \mu\acute{\eta}$; à $\epsilon\iota \omicron\upsilon$ correspondrait en latin *nisi*, à $\epsilon\iota \mu\acute{\eta}$ *si non*. Elle applique sa théorie aux protases conditionnelles, d'abord

quand le verbe est à l'indicatif ('réel'; Mlle K. distingue 'cas actuel', 'suppositions', 'espoir', 'crainte', etc.) ensuite quand il est au subjonctif ou à l'optatif. Puis, elle examine la structure de ἄλλ' εἰ μοί τι πίθοιο, τό κεν πολὺ κέρδιον εἴη (Hom. H 28). Enfin, elle étudie l'irréel.

Il faut avouer que l'auteur n'a pas réussi à emporter notre conviction. Certes, il est vrai qu'en général, la négation μή a un caractère plus accentué que οὐ (voir A. C. Moorhouse, *Studies in the Greek negatives*, Cardiff, 1959, p. 18-32, ouvrage que Mlle K. ne connaissait pas encore), mais ce n'est pas là leur différence essentielle: qu'on pense, par exemple, à l'opposition οὐκ οὖν/οὐκοῦν, dont le premier terme contient un οὐ fortement accentué. Il est d'ailleurs difficile de déceler l'argumentation de l'auteur, qui ne fait guère que donner un ensemble assez confus de phrases prises dans toutes les langues et interprétées à son gré. Prenons pour exemple Hérod. 1, 212: Tomyris fait une proposition à Cyrus, et poursuit: εἰ δὲ ταῦτα οὐ ποιήσεις „si tu ne le fais pas”; Mlle K. (p. 68) veut accentuer 'fais' pour le besoin de sa cause, mais il est beaucoup plus naturel d'accentuer ici la négation. Ce genre d'interprétation étant très subjectif, il est d'autant plus grave qu'elle néglige les indices formels, p. ex. le fait que la négation d'une principale à l'indicatif est toujours οὐ, sauf dans des serments et une catégorie de phrases interrogatives, que la négation d'un infinitif déterminé par l'article est toujours μή, que εἰ οὐ à côté de εἰ μή se rencontre presque exclusivement quand le verbe est à l'indicatif (condition „réelle”). En outre, il importe de distinguer les différents auteurs: l'emploi de εἰ οὐ, p. ex., semble être plus fréquent chez Homère et Hérodote qu'en prose attique. Pour de telles recherches, il faut avant tout établir des tableaux statistiques de l'emploi linguistique.

Voici quelques observations de détail: p. 18: Mlle K. cite Hom. A 137 εἰ δέ κε μή δώσωιν, où Agamemnon fait une supposition opposée à son désir, pour prouver que la protase conditionnelle n'est pas issue d'un vœu; cette argumentation est fautive, car elle confond le plan synchronique avec le plan diachronique: la forme du vœu peut bien être l'origine de celle de la condition. – p. 20: Le rattachement de εἰ au thème *e-/o-* est loin d'être sûr: le synonyme αἰ ne peut guère s'expliquer que comme interjection. – p. 22: Il n'est pas facile de rapprocher le suffixe du locatif (-i bref) de la particule déictique (-i long accentué en grec). – p. 30: Mlle K. prétend que les expressions des langues anciennes étaient plus vagues que celles des langues modernes; on s'étonne qu'

elle n'ait pas été frappée plutôt par la richesse de nuances qu'exprime, p. ex., le système des particules grecques. – p. 32: Il est peu probable que εἰ ait jamais eu la fonction de οὖν et que οὖν l'ait supplanté; d'ailleurs, οὖν n'est pas une particule fréquente chez Homère (85 ex., contre près de 2000 ex. de ἄρ(α)/ῥα). – p. 59: Grâce à son mépris des règles grammaticales, Mlle K. a fait un contresens énorme en traduisant Trag. Adesp. 80 Ν εἴπερ γὰρ οὐδὲ τοῖς κακῶς δεδρακόσιν/ἀκουσίως δίκαιον εἰς ὀργήν πεσεῖν/οὐδ' ἂν ἀναγκασθεῖς τις εὖ δράσῃ τινά/προσῆκον οἴμαι τῷδ' ὀφείλεσθαι χάριν „for if somebody neither involuntarily loses his temper, justly, at evil-doers, nor can be forced to do well, then methinks it is fitting to be grateful to him” au lieu de „s'il n'est pas juste de se laisser aller à la colère envers qui nous a fait du mal contre son gré, celui qui nous a fait du bien parce qu'il y était forcé n'a droit à aucune reconnaissance” (Dufour), ἄν étant = ἐάν. – p. 124: Mlle K. considère ὅτι dans ὅτι μὴ „excepté” comme la conjonction; en réalité, il s'agit du pronom relatif indéfini: ὅ τι μὴ = εἴ τι μὴ „si ce n'est...”.

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J. H. A. ELEMANS, *Woord en wereld van de boer. Een monografie over het dialect van Huisseling*. Uitgeverij Het Spectrum, Utrecht-Antwerpen, 1958. f. 15.—.

Diese Abhandlung, eine Dissertation der Universität von Nimwegen, gibt eine der seltenen Beschreibungen eines Nordbrabanter Dialekts, namentlich das Dialekt aus dem Geburtsort des Autors, aus Huisseling, einem Kirchdorf an der Maas mit etwa 500 Einwohnern, hauptsächlich Kleinbauern. Diese agrarische Gemeinschaft erleidet jetzt mancherlei Änderungen, äussere und innere, durch die Modernisierung der landwirtschaftlichen Technik und weil Lebensmuster und Lebensstil der Jugend angepasst werden an die Entwicklung in der Umgebung.

Die Einsicht des Autors: eine Mundart, eine Heimatkultur geht zugrunde, hat ihn angeregt soviel wie möglich jenes Dialekts und jener Kultur zu sammeln. Sein Material – etwa 6,000 Wörter – gibt er wie ein Berichterstatter in sehr lesbaren Aufsätzen über Natur und Technik, Vieh und Feld, Bauer und Nachbarschaft, also nicht nur alphabetisch. Er erzählt wie man wohnt und arbeitet, welcher Geräte man

sich bedient usw., und gebraucht dabei die typischen Wörter der Mundart. Elemans hat seine Botanisierbüchse gierig gefüllt in Feld und Flur und am Wege, aber die Determination des Gefundenen ist kaum unternommen worden. Es ist charakteristisch für dieses Buch dass es keine Fragen stellt, und nirgends zu einer Folgerung führt. Mehr Überlegung wäre dem Buch zugute gekommen und hätte dem Autor Arbeit und dem Leser an mancher Stelle Zweifel und Warumfragen erspart. Warum z.B. auseinandergesetzt wie man das Alter eines Pferdes aus den Zähnen liest? Diese internationale Methode hat ja nichts mit Mundart oder Heimatkultur zu schaffen; ausserdem ist dieser Gegenstand in Schulbüchern besser behandelt worden, ebenso wie z.B. die Genealogie des niederländischen Schweines. Dass die Mundartbeschreibung sich nicht auf die lokalen Wörter beschränken darf sondern die Wortbestände völlig zu inventarisieren hat ist schon lange anerkannt worden. Ist damit aber der Unterschied zwischen der lokalen Sprache und der nationalen Sprache ganz bedeutungslos geworden? Elemans hat dem Verhältnis zwischen seiner Sprache und der nationalen Sprache kein Gedanke gewidmet. Dieses geht soweit dass er oft Wörter aus der allgemeinen Sprache anbietet als gehören sie nur zu dem Dialekte. Er behandelt *sliet* ganz wie ein Dialektwort (bezug nehmend auf ein Dialektikon aus 1898!) aber scheint wirklich nicht zu wissen dass es ein allgemeines niederländisches Wort ist (synoniem: *rik*, nicht *spar* wie E. notiert). Ebenso: *avveseere*, *bats*, *batteng*, *geheng*, *pieneut*, *teute*, *putter*, *smiespele*, *ronsels*, *kween*. Das Wort *riester* erklärt er mit "strijkbord, stulpijzer", aber er nennt nicht das fast einzige und allgemeine Fachwort für diese Sache *rister*; *broes* umschreibt er mit „bruiser“ (eigenes Fabrikat?), während das allgemeine Wort *broes* ist; *kletseneere* (E.: „praten“), AN Volkspr. *klassineere*; *eus* (E.: „dakrand“), AN *ozing*; Südniederl. *euzie*. Er nennt als typische Mundartaussprache *kiepe* (umkippen) gegenüber AN *kippen*, aber *kiepe* is AN; *dompen* kennt er nur aus einem Dialektikon, nicht im AN; *slof* (kleines Brett unter einem Pfahl) wird umständlich beschrieben, aber nicht gemeldet wird dass es ein nationales Fachwort ist. Dieses Buch über Arbeit und Leben des Bauerns zeigt auch viele agronomische Fehler. Zum Beispiel: *diep* bedeutet nicht nur „mit kurzen Beinen“; dieses allgemeine Fachwort deutet ein gewisses Verhältnis zwischen Beinhöhe und Rumpf an; *rejp* (AN *rijp*) bedeutet nicht „von maximaler Grösse“ sondern „geschlechtsreif“; die Definitionen der Fachausdrücke *binnen-*

beer und *inscharen* sind mangelhaft. An S. 100 erklärt der Autor dass der Unterschied zwischen *dam* und *dijk* nur noch „historisch“ ist, obgleich die Wasserbaukunst heute wie früher beide Ausdrücke genau unterscheidet.

Dem alten hölzernen Pfluge hat E. fast ein ganzes und interessantes Kapittel gewidmet. Aber auch hier zeigt sich das Übel, dass die Konzentration auf das lokale Phänomen geführt hat zu Verengung des Gesichtskreises. Dass der beschriebene Typ in der Landwirtschaftstechnik *karploeg* heisst wusste E. offenbar nicht, aber auch nicht dass von diesem Typ in Niederland vier Abarten bekannt sind mit ihrer regionalen Verbreitung. Und es wäre auch dialektologisch wichtig gewesen wenn der Autor bei dem Worte *meejdorser* erwähnt hätte, dass es zurückgeht auf *maaidorser*, ein Wort das erst vor wenigen Jahren von einer Sprachkommission geschaffen wurde.

In seinem Vorwort erklärt E. dass er sich nicht viel um die „Huis-selinguistiek“ bemüht hat. Grammatik und Syntax sind nicht behandelt worden, wohl aber die Phoneme. Leider, eine Übersicht des Phonemsystems fehlt und mit der Annahme dass jede spezielle Buchstabierung ein Phonem bedeute, würde man 32 Vokalphoneme zählen. Eine wirklich überraschende Anzahl, aber E. kennt nicht den Unterschied zwischen Phonemen und Phonemvarianten, geschweige denn dass er kombinatorische Varianten unterscheidet. Dieses Buch zeugt von einer grossen, fast romantischen Liebe des Autors für seinen Geburtsort, aber jene Liebe hat ihn blind gemacht für diese Tatsachen: dass die lokale Begrenzung nur künstlich ist, ein Eingriff in die Realität; und dass der in Huisseling gesammelte Wortschatz nicht so wie so typisch ist für dieses Dorf.

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J. J. M. BAKKER

N. WIJNGAARDS, *Adaptatie bij de kritische studie van grotere literaire gehelen*. Voordrachten gehouden voor de Gelderse leergangen te Arnhem. Nr. 2. Groningen, 1959.

The problem introduced by Dr. Wijngaards in his lecture is fundamentally a methodological one: the question as to how far a literary study which is not restricted to the drawing up of formal characteristics

is to be necessarily subjective. The author distinguishes between a study of one single literary work and the studies of the complete works of an author, of a group of authors, of a literary period, and indeed, even of all western civilization.

In the study of one single literary work, he argues, there should be "an actual re-creation of the work of art on the part of the student". In a survey of more extensive groups this re-creation is not possible, as it is not a matter of one creation, but in this case subjectivity appears as a result of adaptation, so that the scholar is compelled to make a selection based on subjective grounds. A great deal of attention is given to studies trying to describe and to characterise such larger groups and the interrelation between the literary works of different nationalities. There is no sense in summarising them here. Some works are considered less successful owing to excessive subjectivity. But we wonder how this excess of subjectivity can possibly be determined objectively if the necessity of subjectivity is accepted.

Mr. Wijngaards is very outspoken when he states that the scholar "who does not work upon the principle of style and consequently does not start from the work itself, but from a set of standards, from a preconceived set of criteria by which the work is to be examined, does not interpret but 'stencils'".

But, according to the author, starting from the work itself often admits of many interpretations. This is – we think – a matter of course, for does the re-creation of a work of art start from the work itself or rather from the scholar? Might not we expect more from the development of linguistic studies which may provide the means to objective determination of the communication the text makes?

There is a great risk of terminological confusement if an analysis of an artistic product is called a re-creation and this is virtually incorrect: it is not the creation itself but the result of creating that is investigated, unless the actual creative process is taken as the object of study, but even in that case no re-creation will take place. We would prefer to use the term *creative analysis* of Mr. W. Gs Hellinga and Mr. H. van der Merwe Scholtz. Equally disputable is Mr. Wijngaards' statement that in interpreting a literary work "one should appeal to other faculties besides inducing reason". Even if this statement were true, it ought not to be an argument in the record of the study. The value of the result is partly to be found in the possi-

bility of checking the possible subjective interpretation on account of the facts produced.

But is it right to draw a line of division between a single work and larger groups of works, as Mr. Wijngaards does? His only argument is this doubtful re-creation. Does not a selection of instances that are considered relevant underlie any record of an investigation? This need not present any difficulties as long as the selection is implicitly or explicitly accounted for. It is for other students to determine if or in what respects the scholar has failed.

One of the studies of "larger groups" greatly admired by Mr. Wijngaards is Curtius' *Europäische Literatur und Lateinisches Mittelalter*, but he does not consider the work of practical use for the history of ideas because of the prevalence given to form. We do not think this criticism is altogether fair on Mr. Curtius and we ask ourselves if, for the time being, a literary scholar could do more than make an attempt to typify a period on account of its relevant characteristics. This typification is necessary in order to be able to place the work in the context of a certain period, though it is the study of each separate work that is to provide the material. As long as it proves to be so difficult to determine the communication given by a single text – the author gives some striking examples which may be added to by any careful reader from his own experience – the question seems to be justified whether summaries of "larger groups" might not be premature except in the way of Mr. Curtius'.

These notes may show how many fascinating problems have been introduced by Dr. Wijngaards in his lecture. We are very much obliged to him for his share in the discussion.

University of Amsterdam

F. LULOFS

KAZIMIERZ BULAS and FRANCIS J. WHITFIELD, *The Kościuszko Foundation Dictionary English-Polish, Polish-English. Vol. I: English-Polish*. The Hague, Mouton & Co., 1959.

There are sources according to which between 960 and 963 the first historic Polish prince, Mieczysław (Mieszko) I, came to power; in 966 he accepted Christianity. That meant the emergence of Poland as a

state, its transition from legend to history. Poland has made and is making elaborate preparations to celebrate this millennium, and so does Polonia, i.e., the Polish emigration.

The Kościuszko Foundation, center of Polish culture in America, has undertaken to publish a "Millennium Series", of which this dictionary is to form the cornerstone. Compiled by an American Pole and a well-known American slavist, it constitutes at the same time a cornerstone of Polono-American cultural interrelations and is an important resource for polonists, translators etc. That the work is meant in the first place for users whose mother tongue is Polish is to be seen by the fact that explanations and directions are given in Polish (an inexplicable exception is formed by two non-Polish words in the list of used abbreviations: *obscene* and *obsolete*); only for grammatical terms the English nomenclature is used.

When we consider that in the USA alone several millions of Poles are living, we realize that a dictionary like this supplies a want. It is the first dictionary for that purpose which has this size (the first volume runs to 1037 pages). The undertaking seems to us important enough to pay attention to it already at this stage, although only one of the two volumes has been published (the Polish-English part is soon to follow).

It is the first time an English-Polish dictionary gives on a large scale indications (by adding "Br." or "US") that a word or expression is typical British or American. This seems to us very valuable. The Bulas-Whitfield dictionary distinguishes itself by the great number of American, especially colloquial and slang expressions it contains, which are lacking in many usual English dictionaries. It gives not only translations of words, but also expressions, turns of speech, etc. The way verbs are constructed is indicated (though not always completely; e.g. after *dissolve* we find an example from which it appears that this verb is constructed with *in*, but the possibility *dissolve into* is not mentioned).

Composed words are also given, but here arise difficulties, because it is almost impossible to determine what is a compound and what is not: in English almost every combination of two nouns, simply put together, is possible. It is, therefore, inevitable that inconsistent or unsatisfying cases occur. To take one example: the word *potato* is given, but not the numerous compounds (*potato-blight*, *-box*, *-flour*,

-growing, -lifter, -masher, -mill, -peel, -peeler, -rot, -skin, -trap, etc.). Under *sword* we find *sword dance* (although this combination can easily be understood, if need be by looking up *dance*), but not, e.g., *sword bill* (a bird), *sword flag* and *sword grass* (plants).

Irregular verbal forms are mentioned behind the infinitive form, and besides as words apart, with reference to the infinitive. Pronunciation and stress of every word are indicated, and in this respect too the Anglo-American differences are observed: we find, e.g., four possibilities of pronouncing *buffet* and *cabaret*, etc. A number of geographic and Christian names are entered – apparently, mainly those geographic names which differ from the forms in Polish or in the original language (The Hague is included, Chicago not; but why then is Berlin included?), and mainly Christian names having a Polish equivalent (Robert and Henry are entered, Walter and Humphrey not; Margaret is, Joyce not, etc. – but here too cases of inconsistency occur).

The dictionary is not only very carefully written (this applies both to the English and the Polish text): its preparation lasted for nearly twenty years. But it is also carefully edited (I did not discover any misprint). In our opinion, there might have been more variety in the types used, e.g. by composing the English and the Polish texts with different types. This applies especially to words with many meanings, derivations etc., as, for example, the verb *go*, which occupies $4\frac{1}{2}$ columns: the text might in such cases be more conveniently arranged. As nearly every English noun can also be used as a verb, it should be clearly visible where the treatment of the word as a verb begins. But these are minor remarks, detracting nothing from the merits of this work.

University of California, Los Angeles

T. ECKMAN

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